

AN EXAMINATION OF SHARE PRICE BEHAVIOR
DURING THE 1973-1976 LESSEE ACCOUNTING
STANDARD-SETTING PROCESS

BY

ROBERT B. THOMPSON II

A DISSERTATION PRESENTED TO THE GRADUATE COUNCIL
OF THE UNIVERSITY OF FLORIDA IN
PARTIAL FULFILLMENT OF THE REQUIREMENTS
FOR THE DEGREE OF DOCTOR OF PHILOSOPHY

UNIVERSITY OF FLORIDA

1984

ACKNOWLEDGEMENTS

I would like to thank the members of my committee for their advice, and above all for their patience: Bipin Ajinkya (Chairman), A. Rashad Abdel-khalik (Cochairman), Robert Radcliffe, and E. Dan Smith.

P. C. Venkatesh helped me to unravel the mysteries of computing, and Sarah Terry typed each manuscript with unflagging cooperation.

For purely personal reasons, special thanks are also due to Joan Cartier, Eugene T. Martin, and Alan G. Mayper.

Financial support for this dissertation was provided by the Ernst and Whinney Foundation in the form of an Ernst and Whinney Doctoral Dissertation Award. This support was invaluable, and I am grateful to have received it.

TABLE OF CONTENTS

	PAGE
ACKNOWLEDGEMENTS.....	ii
LIST OF TABLES.....	v
ABSTRACT.....	vii
CHAPTER	
1 INTRODUCTION.....	1
1.1 Notes.....	10
2 EXISTING STUDIES OF THE SHARE PRICE IMPACT OF LESSEE ACCOUNTING AND REPORTING STANDARDS.....	11
2.1 Introduction.....	11
2.2 Recent Evolution of Lessee Financial Accounting and Reporting Standards.....	12
2.3 The Financial Statement Impact of Lease Capitalization.....	25
2.4 Review and Analysis of Previous Studies.....	27
2.5 Summary.....	51
2.6 Notes.....	53
3 THE CONTRACTING HYPOTHESIS.....	56
3.1 Introduction.....	56
3.2 Leftwich Study.....	57
3.3 Accounting-Based Loan Covenants and Lease Capitalization.....	62
3.4 Factors Influencing the Cross-Sectional Distribution of "Contracting" Share Price Effects.....	70
3.5 Hypotheses.....	90
3.6 Notes.....	93
4 SAMPLE SELECTION AND PRELIMINARY TESTS OF EVENT-RELATED ABNORMAL PERFORMANCE.....	95
4.1 Introduction.....	95
4.2 Sample Selection.....	96
4.3 Data.....	98
4.4 Research Design for First-Stage Tests.....	105
4.5 Empirical Results.....	112
4.6 Interpretation and Discussion.....	155
4.7 Notes.....	164
5 TESTS OF THE CONTRACTING HYPOTHESIS.....	165
5.1 Introduction.....	165
5.2 Restatement of Hypotheses.....	166
5.3 General Description of Test Procedure.....	168

CHAPTER	PAGE
5.4 Description of Sample and Data	
Collection Procedures.....	174
5.5 Measurement of Independent Variables.....	177
5.6 Results.....	179
5.7 Robustness of Results.....	192
5.8 Notes.....	205
6 SUMMARY AND CONCLUSIONS.....	206
APPENDIX DERIVATION OF BIAS EQUATIONS.....	215
REFERENCES.....	217
BIOGRAPHICAL SKETCH.....	223

LIST OF TABLES

TABLE NUMBER		PAGE
4-1	Summary of Sample Selection Procedure.....	99
4-2	Distribution of TA and (NCL/TA).....	100
4-3	Index of Events Connected with the Evolution of Lessee Accounting Standards.....	103
4-4	Event Weeks and Event Periods.....	110
4-5	Distribution of "Market Model" Regression Estimates.....	113
4-6	Estimated Coefficients of Cross-Sectional Regressions - Event Weeks 1-16.....	115
4-7	Estimated Coefficients of Cross-Sectional Regressions - Event Periods 1-11.....	117
4-8	Summary of Estimates $\hat{a}_{3t}$ from Cross- Sectional Regressions.....	119
4-9	Summary of Weekly Return Observations Used in Estimating "Local" β_j	133
4-10	Distribution of "Local" β -Estimates.....	137
4-11	Estimated Standard Errors of "Prior" and "Local" β -Estimates.....	139
4-12	Variance of Market Model Disturbance Term - "Prior" and "Local" Estimates.....	140
4-13	Revised Regression Results - Event Weeks.....	143
4-14	Revised Regression Results - Event Period Intervals.....	146
4-15	t-Statistics and Estimated Standard Errors - Event Period Intervals.....	148
4-16	Confidence Intervals and t-Statistics for $\sum_t n_t \hat{a}_{3t}$	154
4-17	Comparison of Results.....	160

TABLE NUMBER		PAGE
5-1	Partitioning Scheme.....	167
5-2	Independent Variable Definitions.....	180
5-3	Regression Variables - Descriptive Statistics.....	181
5-4	Regression Variables - Product Moment Correlations.....	182
5-5	Cross-Sectional Regression Results.....	184
5-6	Alternative Specification of Dependent Variable.....	196
5-7	Alternative Specification of λ'	197
5-8	Alternative Specification of D_j	198
5-9	Industry Distribution in Total Sample Regressions.....	199
5-10	Alternative Specification with Industry Dummy Variables.....	200
5-11	Lessee Subsample Only.....	201
5-12	Lessee-Only Regressions - Implied Cumulative Abnormal Returns.....	203

Abstract of Dissertation Presented to the Graduate Council
of the University of Florida in Partial Fulfillment of the
Requirements for the Degree of Doctor of Philosophy

AN EXAMINATION OF SHARE PRICE BEHAVIOR
DURING THE 1973-1976 LESSEE ACCOUNTING
STANDARD-SETTING PROCESS

By

Robert B. Thompson II

April 1984

Chairman: Bipin B. Ajinkya
Cochairman: R. Rashad Abdel-khalik
Major Department: Accounting

Generally accepted accounting principles, the financial reporting rules followed by U.S. business entities, are continuously modified by agencies such as the U.S. Securities and Exchange Commission and the Financial Accounting Standards Board. This process occasionally flares into controversy when it is felt that the financial statements of a particular class of firms will be adversely affected by a contemplated rule change. It is sometimes claimed that the accounting effects of the rule change will result in a downward revision of the equity values of affected firms. One argument offered in support of such claims is that a mandated accounting change may effectively alter the degree to which shareholder activities are restricted by accounting-based negative loan covenants. An alteration of this type may result in a transfer of wealth from shareholders to creditors, or in deadweight losses born primarily by shareholders, as

resources are consumed in coping with the contract-related effects of the change.

In this dissertation, the behavior of share prices during the 1973-1976 "lessee accounting" standard-setting process is examined for consistency with the above arguments. This policy process culminated in 1976 with the issuance, by the Financial Accounting Standards Board, of Statement of Financial Accounting Standards No. 13 - Accounting for Leases. In some cases this accounting standard, which mandated the capitalization of a class of "financing leases," caused non-trivial changes in financial statement relationships frequently used as a basis for loan covenants.

Subject to certain caveats, the data examined are consistent with the hypothesis that lessee shares were valued downward (relative to non-lessee shares) as a result of the process. The observed cross-sectional pattern of returns is consistent with two predictions which are deduced from the "contracting hypothesis" described above. Firms which are highly leveraged, and for which the potential financial statement impact of the standard was large, exhibited significant negative abnormal price behavior relative to other groups of firms. And within this class of firms, the data suggest a significant relationship between the magnitude of abnormal performance and the proportion of long-term debt actually subject to accounting-based restrictions.

CHAPTER 1

INTRODUCTION

Beginning with the burgeoning popularity of the "sale-and-leaseback" in the late 1940s and early 1950s, and continuing through to the present day, the long-term noncancellable lease has grown steadily in importance as a financing vehicle. During most of this same period, the question of appropriate financial reporting for these "financing leases" has been a matter of considerable debate. One faction in this controversy -- lessee firms and their representatives -- has generally favored treating leases for financial reporting purposes as simple period-by-period rental arrangements, on the theory that lease contracts, like wage contracts and purchase commitments, are strictly executory in nature. Another faction, peopled largely by academics, financial analysts, and (to a limited extent) financial accounting policy makers, has maintained that, regardless of their outward form, financing leases are in substance asset acquisitions financed by borrowing, and should be accounted for as such. And in the region separating these extremes, many shades of accounting argument have been voiced over the years.¹

In terms of actual financial reporting practice, the "rental" approach to lease accounting was used almost exclusively by lessees

until the late 1970s. A slow shift in policy orientation toward mandatory capitalization began in April 1973, when the newly organized -- and soon to be official -- Financial Accounting Standards Board (FASB) announced a comprehensive review of lease accounting as one of seven items on its initial agenda. In its own last official pronouncement, Accounting Principles Board Opinion No. 31, the Accounting Principles Board moved in June 1973 to standardize the form and content of disclosure by lessees. In October of the same year, the U. S. Securities and Exchange Commission (SEC) issued Accounting Series Release No. 147, which in essence amended Regulation S-X to include the disclosure requirements of APB 31, plus additional requirements for the disclosure of pro forma income statement and balance sheet effects based on an assumed set of capitalization rules. Following these actions, the FASB entered upon its own standard setting process, which culminated almost three years later, in November 1976, with the issuance of Statement of Financial Accounting Standards No. 13 -- Accounting for Leases (SFAS 13). From the lessee's perspective, the general thrust of this new standard was to extend the subset of leases (some would say to create a subset of leases) for which capitalization is the required accounting treatment. Moreover, in an almost unprecedented fashion, the Board held the new capitalization provisions to be retroactively applicable to existing leases. Thus, for many lessees, the financial statement impact of SFAS 13 was dramatic.

The standard setting process just outlined was heatedly contested at every step along the way, and, as it became increasingly evident that the final outcome of the process would be an incrementally stringent capitalization requirement, the policy debate centered more and more on welfare-type arguments than on the technical accounting arguments mentioned earlier. Proponents of capitalization (including the SEC) argued for the welfare of the investor, citing his need for comparable financial statements and maintaining that similar transactions (i.e., asset acquisitions financed using conventional loans and leased acquisitions) should receive similar accounting treatment in order to minimize confusion. Opponents argued that mandatory lease capitalization would, for a large number of lessees, so adversely affect key financial statement relationships that access to capital would be impaired. The costs to shareholders of such firms were held to be so great as to outweigh any potential benefits of capitalization to investors, particularly in light of the fact that full and uniform disclosure of leasing activities had been achieved with the promulgation of APB 31 and ASR 147. Any investor who felt that capitalization was the appropriate accounting treatment for financing leases could, it was argued, easily adjust a firm's financial statements using the data disclosed in compliance with these standards.

The broad purpose of the present dissertation is to examine whether the behavior of lessee share prices during the 1973 - 1976 "policy change" period was consistent with the predictions of those

who opposed mandatory lease capitalization. This question should be of particular interest to the accounting community, since the anti-capitalization arguments referred to above imply that it is the manner in which information is disclosed, rather than the information per se, which is responsible for the predicted adverse effects.

At the time, this notion was greeted with a degree of skepticism by some accountants, and in particular by many academics. The perception of the present author is that, throughout most of the 1970s, a preponderance of accountants tended to view the financial accounting process as being primarily "informational" in nature.² In the academic community, this informational perspective was coupled with an almost rigid belief that securities markets are extremely efficient in the semi-strong form defined by Fama (1970). A joint implication of the informational perspective and semi-strong form efficiency is that security prices should be invariant, under a reasonable range of conditions, to the form in which data are publicly disclosed. According to a well-known exposition of the implications of market efficiency for accounting written during the period in question,

Since information is publicly available even if it is only reported in a footnote to the published financial statements, or in a 10-K annual report to the Securities and Exchange Commission, market efficiency would predict that such information would be properly impounded in security prices. Further, any subsequent disclosure of such information in a more visible manner . . . would not cause the security's price to change. (Dyckman, Downes and Magee, 1975, p. 86)

Under this view, and given the existing disclosure regulations, lease capitalization would add no new information to the public domain, and hence would have no impact on prices.

The counterargument, presented vigorously by lessees at every opportunity, can be paraphrased as follows. The financial accounting process actually serves a dual purpose. On the one hand, it provides information which is useful in assessing the risk and return characteristics of individual securities or other financial claims, and viewed solely from this perspective, the value of such claims should be invariant to the form in which data are publicly disclosed. On the other hand, its output (periodic financial statements) is the product of reasonably stable measurement rules (i.e., generally accepted accounting principles), and is subject to verification by independent third parties. Thus, financial statement numbers and relationships have also provided a basis on which these same claimants can form contracts, and, where such accounting-based contracts are actually in force, the invariance of price to financial statement content can no longer be asserted. In the specific instance of lease accounting, one impact of mandated capitalization on firms which are significant

lessees is an adverse, and sometimes radical, transformation of balance sheet numbers (e.g., the level of retained earnings) and relationships (working capital, the debt-equity ratio, the ratio of net tangible assets to funded debt) which are commonly employed as control devices in loan contracts. In the case of a lessee firm which is bound to comply with contract terms of this type, capitalization can lead to an immediate technical contract violation or, at a minimum, to an increase in the probability that such a violation will occur in the future. If the penalty for violation is severe, the firm's shareholders must bear the cost of renegotiating the affected contract or, alternatively, of revising investment and financing programs to compensate for what amounts to an unanticipated increase in the restrictiveness of the contract.

The intuitive appeal of this counterargument leads directly to a secondary purpose of the present study. If lessee share price behavior during the policy change period is consistent with the hypothesis that lessee equities were adversely affected by the change, it then becomes a matter of interest to inquire whether the cross-sectional pattern of price behavior is reflective of the "contracting" notion on which the counterargument is based.

Some results on this question are already available. For example, Lev (1979) and Collins and Dent (1979) provide evidence that the equities of firms using the "full cost" method of accounting for oil and gas exploration costs were valued downward (at least in relative terms) during the standard setting process leading to the

issuance of Statement of Financial Accounting Standards No. 19 -- Financial Accounting and Reporting by Oil and Gas Producing Companies (FASB, 1977).³ This standard (later withdrawn), which mandated use of the "successful efforts" method, would have resulted in balance sheet and income statement effects similar to those of SFAS 13. Moreover, a cross-sectional analysis by Collins, Rozeff and Dhaliwal (1981) indicates that the adverse price behavior was more severe on average for full cost firms having accounting-based loan contracts outstanding. In a different vein, results presented by Leftwich (1981) indicate that the equities of "merger-prone" firms were valued downward during the deliberations leading to the adoption of Accounting Principles Board Opinion No. 16 -- Business Combinations (1970), and a cross-sectional analysis is again suggestive of a "contracting"-type explanation. In both of these studies, however, the evidence in support of the contracting hypothesis is relatively weak, and it is hoped that the present study will shed additional light on the question.

The whole question of whether non-informative (or not very informative) changes in the form and content of financial statements can have a material impact on security values is one of more than purely academic interest. Agencies charged with the responsibility for setting financial reporting standards must mediate among the interests of all parties who may be affected by their decisions regarding specific accounting policy issues. The argument that shareholders of class x would be adversely affected by policy decision y as a result

of y's impact on accounting numbers used in loan contracts has become quite a common one, and the FASB in particular has shown a tendency to be responsive. In connection with the retroactive application provision of SFAS 13, for example, the Board established a four year "transition period," an express purpose of which was to allow firms to renegotiate any loan contracts which would be seriously affected by the capitalization of existing leases. Moreover, a glance at the Board's current agenda reveals at least two incipient issues -- pension accounting and accounting for income taxes -- whose resolution could conceivably have as dramatic a financial statement impact as did SFAS 13, and in which contracting arguments are likely to play a critical role. As indicated earlier, however, empirical evidence in support of such arguments is rather slender, and it is felt that, by contributing to the present body of evidence, this study should be of at least indirect value to accounting policy makers.

The remainder of this dissertation is organized as follows. Chapter 2 provides a more detailed description of the 1973-1976 lessee accounting standard setting process, a brief discussion of the financial statement impact of lease capitalization, and a critical analysis of some existing studies of the share price impact of APB 31, ASR 147, and SFAS 13. The "contracting hypothesis" is explicated in Chapter 3. Included therein is a discussion of the use of accounting constraints in loan contracts, the effect of lease capitalization on

the firm's accounting position with respect to such constraints, and the cross-sectional pattern of share price behavior which might be expected as a consequence. The results of empirical tests are reported in Chapters 4 and 5. Chapter 4 addresses the question of whether lessee share prices were valued downward (relative to those of non-lessees) during brief intervals surrounding a number of events which occurred during the course of the 1973-1976 standard setting process, and Chapter 5 examines whether the observed "abnormal performance" corresponds to the cross-sectional pattern predicted in Chapter 3. Finally, in Chapter 6 the study as a whole is briefly summarized, some limited conclusions are stated, and some suggestions for future research are made.

1.1 Notes

1. The history of the controversy over appropriate lessee accounting is described in greater detail in Chapter 2. For an excellent review of the technical accounting issues involved, see Financial Accounting Standards Board (1974).
2. Under an "informational" perspective on the financial accounting process, the primary purpose of periodic financial statements is to provide external users with data which can be used in assessing the risk and return characteristics of the various financial claims against the reporting firm. This perspective is perhaps best exemplified by the spirit of the Report of the Study Group on Objectives of Financial Statements (American Institute of Certified Public Accountants, 1973), more commonly known as the Trueblood Report.
3. For a different interpretation of the evidence, however, see Dyckman and Smith (1979).

CHAPTER 2
EXISTING STUDIES OF THE SHARE PRICE
IMPACT OF LESSEE ACCOUNTING
AND REPORTING STANDARDS

2.1 Introduction

As indicated in Chapter 1, a considerable controversy surrounded the adoption, during the mid-1970's, of APB 31, ASR 147, and SFAS 13. The first and second of these standards, promulgated respectively by the Accounting Principles Board and the Securities and Exchange Commission, had the effect of standardizing and expanding the set of disclosures required of lessee firms. The third standard, issued by the Financial Accounting Standards Board, required in addition that lessees capitalize leases meeting certain criteria.

Opponents of these standards argued that increases in the required level of disclosure or in the stringency of capitalization criteria could have a serious impact on the economic position of lessee shareholders. Four existing studies of lessee share price behavior during the APB 31, ASR 147, and SFAS 13 standard setting periods suggest that this may indeed have been the case, although they shed little light on the question of why such effects might have occurred. The primary purpose of the present chapter is to examine these studies in order to see what conclusions can reasonably be drawn from their results. Before undertaking this task, however, some

background information is in order. Accordingly, section 2.2 describes the evolution of standards for financial accounting and reporting by lessees during recent years, ending with the adoption of SFAS 13. Section 2.3 provides a brief discussion of the financial statement impact of lease capitalization. Section 2.4 provides a review of the studies mentioned above, together with a critical analysis of the results reported therein. The chapter as a whole is briefly summarized in Section 2.5

2.2 Recent Evolution of Lessee Financial Accounting and Reporting Standards

While the use of the noncancellable long term lease as a financing device has increased steadily and dramatically since the late 1940's,¹ the financial reporting rules applicable to leases show very little change prior to 1973. However, the question of whether financing leases should be capitalized (i.e., recorded as assets and corresponding obligations) or treated as rental arrangements has been controversial almost from the beginning of this period.² An early position, expressed in Accounting Research Bulletin No. 38 by the AICPA's Committee on Accounting Procedure, was that a lease should be capitalized only if it were "clearly in substance an installment purchase of property" (Committee on Accounting Procedure, 1949, p. 7). Despite subsequent arguments by some writers (e.g., Myers, 1962) that financing leases should be capitalized because they represent property rights, the "installment purchase" criterion was reaffirmed by the Accounting Principles Board in APB Opinion No. 5 (1964).³

ARB 38 was never actually binding on reporting firms, and APB 5, while binding, did not require capitalization of lease contracts written prior to 1965. In addition, companies apparently had little difficulty in writing new lease contracts which avoided the capitalization criteria set forth in APB 5. Therefore, although the practice of leasing grew at a rapid rate throughout the 1950's and 1960's, few leases were actually capitalized.⁴ Moreover, because neither of these pronouncements explicitly required a detailed supplemental disclosure of the significant aspects of noncapitalized lease contracts, financial statements included relatively little information about the leasing activities of reporting firms prior to the mid-1970's. For example, May, Harkins and Rice (1978) found that during the fiscal year preceding the year in which APB 31 (see below) became effective, the lease disclosures of over half of a large random sample of lessees consisted at most of a footnote detailing rental expense for the current and/or past year.⁵

The ultimate resolution of these disclosure and accounting issues began to take shape in 1973, during a period of some confusion in the accounting policy making environment. In July of that year, private sector standard setting authority was formally transferred from the AICPA's Accounting Principles Board to the newly created Financial Accounting Standards Board. And throughout this period, private sector accounting policy formation appears to have been heavily influenced by an increasingly "activist" Securities and Exchange Commission, under the leadership of Chief Accountant John C. Burton.

The disclosure issue was addressed in 1973 by both the APB (Opinion No. 31) and the SEC (Accounting Series Release No. 147). During 1971 and 1972 the APB had already taken some tentative steps toward dealing with lease accounting problems. A public hearing on the subject was held in October 1971, and various draft proposals were privately circulated both before and after the hearing. In April 1973, the FASB prepared an initial agenda which included (among other things) appropriate financial accounting and reporting procedures for both lessees and lessors. Although the Board clearly indicated that it did not intend to consider the issues involved on a piecemeal basis, and intimated that the final lease accounting standard would not be quickly forthcoming, the APB more or less simultaneously announced that its own proposals for lessee reporting rules would be dropped.

In May 1973, an SEC official charged that the accounting profession "has probably failed" [to keep up] "with the phenomenal growth and complexity of lease arrangements," ("SEC Plans to Require," 1973, p. 9) and announced that the SEC was likely to propose a new set of disclosure rules for lessees. Such a proposal was in fact published for comment on June 6, 1973, under the title Securities Act Release No. 5401. SAR 5401 suggested amendments to Regulation S-X which would require not only an increase in the general level of disclosures concerning noncapitalized leases, but also the presentation of pro forma income statement and balance sheet figures calculated under the assumption that certain leased assets had been purchased using borrowed funds.

One week later, and two weeks prior to its own demise, the APB issued Opinion No. 31, which might be regarded as its own "comment" on SAR 5401. Passed with only one dissenting vote, APB 31 required footnote presentation of a detailed schedule of future minimum rental commitments arising from noncapitalized, noncancellable leases, together with a disclosure of rental expense for the current year and a brief description of sundry aspects of existing lease contracts. In framing the opinion, the Board emphasized the interim nature of the disclosure requirements established therein, and pointedly declined to adopt the SEC's suggestion that pro forma capitalization disclosures be mandated:

. . . the Board is refraining from establishing any disclosure requirements which may prejudice or imply any bias with respect to the outcome of the FASB's undertaking, particularly in relation to the questions of which leases, if any, should be capitalized and how such capitalization may influence the income statement. Nevertheless, in the meantime the Board recognizes the need to improve the disclosure of lease commitments in order that users of financial statements may be better informed. (Accounting Principles Board, 1973, p. 5)

The SEC, apparently unimpressed either by the APB's position or by the 134 letters of comment (all negative) received in response to SAR 5401, issued ASR 147 in October 1973. ASR 147 formally amended Regulation S-X to require 10-K presentation of (a) the lease data required to be disclosed under APB 31, (b) the discounted value of a subset of lease commitments referred to as "noncapitalized financing leases," and, provided that certain materiality criteria were met; (c) the impact on net income if all noncancellable financing leases were

capitalized, the related assets amortized on a straight line basis, and interest recognized on the outstanding lease liability.⁶ ASR 147 defined a financing lease as one which,

during the noncancellable lease period, either (i) covers 75 percent or more of the economic life of the property or (ii) has terms which assure the lessor a full recovery of the fair market value . . . of the property at the inception of the lease plus a reasonable return on the assets invested. (U.S. Securities and Exchange Commission, 1973b, p. 547)

In its introduction to ASR 147, the Commission provided a very clear statement of its own attitude regarding the adequacy of the APB 31 disclosure requirements and the importance of pro forma capitalization disclosures to investors:

The Commission has carefully considered the contents of Opinion No. 31 to determine whether it provided for sufficient disclosure to meet the needs of investors and has concluded that it does not. . . . Specifically, the Commission believes that disclosures of the present value of financing leases and of the impact on net income of capitalization of such leases . . . are essential to investors . . . [and are] . . . necessary to enable investors to compare meaningfully the capital and asset structures and operating results of companies making use of different methods of acquiring and financing assets. (U.S. Securities and Exchange Commission, 1973b, p. 546)

Having thus defined what, in its own eyes, constituted "adequate disclosure," the Commission concluded its introductory remarks by stating that its intention was not "to prejudge the issues of lease accounting now being considered by the Financial Accounting Standards Board" (U.S. Securities and Exchange Commission, 1973b, p. 546).

SEC spokesmen pointed out that an FASB pronouncement on lease accounting was not expected before 1975, and that "two years was just too long to wait in terms of disclosure" ("SEC Adopts Rule," 1973, p. 8).

By disposing of the disclosure issue, the APB and the SEC cleared the ground for a consideration by the FASB of lease-related accounting issues. During the three year period following the SEC's adoption of ASR 147, the FASB engaged in its by now familiar routine of discussion memorandum, public hearing, and exposure draft. The culmination of this process was Statement of Financial Accounting Standards No. 13 -- Accounting for Leases, issued by the Board in November 1976. The process itself, which is of major significance from the perspective of the present study, is reviewed here.

As indicated previously, the FASB placed lease accounting on its initial agenda in April 1973. In consultation with a broad based independent task force which included representatives from industry, government, public accounting, the financial community, and academe, the FASB staff prepared a discussion memorandum which outlined and analyzed the lease accounting issues to be considered during the standard setting process. These included accounting by lessees and lessors, accounting for sale-and-leaseback and leveraged leasing transactions, and matters of implementation. With regard to lessee accounting, the major questions to be resolved were simple, albeit highly controversial. First: for what subset of the universe of leasing transactions is "capitalization" the appropriate accounting treatment? And second: given a capitalization policy, should that

policy be implemented on a retroactive basis or on a prospective basis? Basically a very neutral document, the discussion memorandum proposed no answers to these questions, but merely outlined the theoretical and popular support for existing schools of thought. Issued in July 1974 under the title An Analysis of Issues Related to Accounting for Leases, and widely disseminated, the discussion memorandum called for letters of comment and position papers, and invited interested parties to present their points of view at a four day public hearing to be held during the following November.

The public hearing was apparently well attended and was the subject of fairly detailed press coverage by a Wall Street Journal staff reporter (Andrews, 1974). Some generalizations can be made concerning the attitudes of the "interested parties" in attendance. Large public accounting firms, professional and academic accounting organizations, financial analysts, and the New York Stock Exchange tended to favor capitalization rules more stringent than those of APB 5, generally on the grounds that the resulting financial statements would be more "useful" or more "comparable" across firms. However, these organizations exhibited some disagreement on the question of retroactive v. prospective application. Lessees and organizations representing lessees tended to oppose capitalization, and, given capitalization, to oppose retroactive application. Although this opposition was sometimes justified using accounting arguments (e.g., it would be inappropriate to impose a lease capitalization requirement without first considering appropriate accounting procedures for the entire spectrum of executory contracts), lessees more often than not

cited economic arguments against capitalization. For example, Robert O. Whitman, representing the Financial Executives Institute, suggested that mandatory capitalization would cause some lessees to be in default under existing loan agreements, and would generally make it more difficult for lessees to raise new capital through borrowing. At the macroeconomic level, such a requirement would "conflict with the nation's goals of combating inflation, fighting unemployment, protecting the environment, and seeking energy self-sufficiency" (Andrews, 1974, p. 18).

Formally, the public hearing was no more than a forum organized to allow interested parties to express their views on the subject of lease accounting. The FASB itself adopted no position either during the hearing or at its close. However, Andrews, the Wall Street Journal reporter, clearly departed under the impression that "the weight of accounting opinion favors capitalizing most long term leases" (Andrews, 1974, p. 18), and that opponents of capitalization had presented arguments which were less than convincing. This was also the assessment of John C. Burton, the SEC's Chief Accountant, who noted that "those opposed [to lease capitalization] sense that they have lost the battle in the accounting profession, and they are now looking to Congress." Mr. Burton warned that the FASB should "be prepared to make a case to Congress" (Andrews, 1974, p. 18).

However clear the "weight of accounting opinion" may have been at the time of the public hearing, a draft version of the proposed final standard was not issued for public comment until late August 1975. In the matter of accounting by lessees, this document (hereafter referred

to as the "first exposure draft") proposed five conditions individually sufficient for capitalization. These conditions, which corresponded very roughly to the ASR 147 "financing lease" definition, were designed to operationalize the fundamental position adopted by the FASB: that a lease should be capitalized if it "transfers substantially all of the benefits and risks incident to the ownership of property" (FASB, 1976, p. 54) to the lessee. Basically, the proposed standard required capitalization of any lease which confers ownership of property on the lessee, either through an outright transfer of title or through a bargain purchase option. Even in the absence of an effective transfer of ownership, however, the proposal required capitalization if the noncancellable lease term equals or exceeds 75% of the economic life of the leased property, if the residual value of the leased property is anticipated to be less than 25% of its fair value at the inception of the lease, or if the leased property is special purpose property to the lessee and not readily saleable to another user.

The apparent intent of the Board in proposing these conditions was to require a level of lease capitalization beyond that implied by the "installment purchase" criterion of APB 5. On the question of implementation, however, the Board proposed that the new capitalization conditions be applied only to lease transactions effected on or after January 1, 1976. In some 240 letters of comment received in response to the first exposure draft, the Board was criticized both by lessees (for proposing an increased level of capitalization) and by proponents of capitalization (for proposing that the capitalization

conditions be applied on a prospective basis). The latter group included the SEC, which argued that it was nonsensical to adopt stiff capitalization rules and at the same time to exempt billions of dollars in existing leases.

In November 1975, Marshall S. Armstrong, the FASB chairman, announced that the Board needed more time to consider the comments received in response to the first exposure draft, and that a final pronouncement on lease accounting rules would not be issued until early in 1976. This projection proved to be somewhat optimistic: the Board's "consideration," which extended throughout the first half of 1976, resulted in alterations of the proposed standard which were felt to be sufficiently significant to warrant yet another opportunity for public comment. In order to allow for this, the Board issued a second exposure draft in July 1976.

In the matter of lessee accounting, the terms of the second exposure draft differed from those of the first in two respects. First, the technical conditions for capitalization were altered. The "special purpose property" condition was dropped entirely, on the argument that such a condition was not itself sufficient to indicate that the risks and benefits of ownership had passed from lessor to lessee. In addition, the "nominal residual value" condition was dropped in favor of an "investment recovery" condition: capitalization would be required if the discounted value of non-cancellable minimum rentals equals or exceeds 90% of the fair value of the leased property (less any investment tax credit retained by the lessor) at the inception of the lease.⁷ Second, and perhaps of primary

importance to lessees, the Board revised its position regarding implementation, proposing retroactive capitalization of existing leases at the close of a four year "transition period." During the transition period, the financial statement effects of retroactive capitalization were to be given footnote disclosure, but early application of the new capitalization rules to all leases was encouraged by the Board.

Many respondents to the discussion memorandum had indicated that retroactive application of stringent capitalization rules would cause some lessee firms to be in technical violation⁸ of the terms of existing loan agreements, and the Board seemed to have this problem in mind when it proposed "prospective application only" in the first exposure draft. In the second exposure draft the Board argued that the four year transition period would give such firms ample opportunity to renegotiate affected contracts prior to the time at which retroactive capitalization became mandatory, and thus to avoid most if not all of the potential costs of compliance. An analysis of letters of comment received by the Board following the release of the second exposure draft suggests that many lessee firms were unimpressed by this argument. Of 116 lessees responding negatively⁹ to the second exposure draft, 63% opposed retroactive capitalization for some reason, and of the latter group, slightly more than half cited "loan covenant problems" as a primary reason for their opposition. A number of respondents argued that a renegotiation of existing loan agreements would entail two types of costs. The out-of-pocket costs of renegotiating some loans -- those publicly held -- would be large,

and, moreover, lenders would be likely to demand interest rate concessions in exchange for a favorable adjustment of accounting-based loan provisions.

Despite these and other more technical arguments related to the form of the capitalization conditions, the Board had apparently decided by this time that a final standard could be issued. In late November 1976, the Board issued Statement of Financial Accounting Standards No. 13 -- Accounting for Leases. The provisions of SFAS 13 -- virtually identical to those of the second exposure draft -- became effective immediately for lease contracts negotiated on or after January 1, 1977, and retroactively for reporting years beginning after December 31, 1980. Although one Board member voted against SFAS 13 in its final form, it is apparent from a discussion of the reasons for his dissent that the Board as a whole was reasonably satisfied with those portions of the standard which dealt with lessee accounting and reporting.

The adoption of SFAS 13 brought the long controversy over lessee accounting to a tentative close.¹⁰ It is evident from the history recited above that the period 1973-1976 was one in which the "rules" for lease-related financial accounting and reporting changed dramatically and, at least in the eyes of many lessees, detrimentally. With regard to APB 31 and ASR 147, some lessees maintained that the mere disclosure of new data concerning their leasing activities could be economically harmful. It was felt that, as a result of such disclosure, investors and creditors would perceive lessees as being more risky, therefore less credit-worthy and, more generally, less

valuable. However, the sharpest criticism by lessees during the APB 31/ASR 147 period was directed at the SEC's requirement for pro forma balance sheet and income statement disclosures calculated under the assumption that "financing leases" had been capitalized. At issue was whether, by mandating these particular disclosures, the SEC was prejudging or predetermining the outcome of the yet-to-begin FASB deliberations on the matter of capitalization by lessees. And it is clear from the history of those deliberations that, with respect to lessee accounting, the thorniest questions faced by the Board were whether leases should be capitalized, which leases should be capitalized, and whether capitalization should be instituted on a retroactive or on a prospective basis. In response to the Board's proposals, lessees were quick to suggest that the financial statement effects of more stringent capitalization rules could impair a firm's ability to raise capital and perhaps result in technical violations of loan covenants. And these effects would be magnified, they claimed, should retroactive application be mandated. A number of lessee respondents argued rather heatedly that it would be inappropriate to apply the Board's capitalization conditions ex post to transactions which would have been structured differently had the new rules then been in effect.

Most of the remainder of this chapter is devoted to a review and discussion of existing studies which have sought to determine whether the adverse effects predicted by lessees during the APB 31/ASR 147/ SFAS 13 period did in fact materialize. However, because these

effects were alleged to flow mainly from the financial statement impact of lease capitalization, that impact is briefly described below.

2.3 The Financial Statement Impact of Lease Capitalization

As indicated above, ARB 38 prescribed that certain leases -- those which were, in substance, installment purchases of property -- should be capitalized, and APB 5 set forth criteria to be used in determining whether a particular lease satisfied this condition. However, the APB 5 criteria were sufficiently vague that, prior to SFAS 13, financing-type leases were typically accounted for simply by charging periodic rental payments against income, with no asset or obligation recognized in the lessee's accounts.

Under SFAS 13, a lease which satisfies any one of the standard's four capitalization conditions is given accounting treatment as a "capital lease." The lessee records both an asset and a liability equal to the present value of the minimum rental payments to be made during the noncancellable lease term. While the asset is noncurrent, the liability will generally include both current and noncurrent components. Expenses recognized under this scheme include depreciation (where applicable) and interest on the unamortized lease obligation. In the very common case of a lease which calls for level rentals, annual expense will decline (in total) year by year, exceeding the rental amount during the early years of the lease and exceeded by the rental amount during later years. Thus, a change from "rental" accounting to capitalization involves both additions to the balance sheet and an accelerated pattern of expense recognition.

For firms with a prior history of leasing, the effects of retroactive capitalization on key balance sheet relationships are largely unambiguous. Indicators of leverage, such as the debt-equity ratio, increase. Indicators of liquidity (working capital, current ratio) and indicators of debt coverage (e.g., the ratio of net tangible assets to funded debt) decline. In addition, because the total earnings accumulated since the inception of the firm's leasing activities will be lower on a post-capitalization basis, both total retained earnings and retained earnings "unrestricted as to dividend payments" should decrease.

The effects of retroactive capitalization on financial statement "flow" relationships are somewhat less predictable, because the impact of capitalization on reported income for a given period depends on the relative age of lease contracts considered in the aggregate. As an empirical matter, however, studies by Ro (1978) and Pfeiffer (1980) suggest that for virtually all firms affected by ASR 147 and SFAS 13, the effect of capitalization on net income is either negligible or negative. This in turn implies a deterioration in financial ratios which measure fixed charge coverage and book rates of return.

One final point regarding the ultimate financial statement impact of SFAS 13 is worth noting. It is apparent from post-SFAS 13 financial reports that many long term leases written during the 1950's, 1960's, and early 1970's, while avoiding the capitalization provisions of APB 5, fell well within the scope of the SFAS 13 capitalization conditions and were therefore subject to retroactive capitalization. On the other hand, because the SFAS 13 capitalization conditions are

highly arbitrary, they appear almost to invite circumvention, and it has been suggested¹¹ that many post-SFAS 13 lease contracts are written close to the "capitalization borderline," thereby achieving accounting treatment as rental arrangements. The latter observation is based on purely anecdotal evidence, but, taken jointly with the former, at least tentatively implies that the standard's hypothetical "adverse potential" results largely from its provision for retroactive application.

2.4 Review and Analysis of Previous Studies

As indicated above, the new financial accounting and reporting requirements imposed by the APB, SEC, and FASB elicited a response from lessees which can only be described as hostile. Lessees who opposed the new standards tended to offer equity and welfare arguments, rather than accounting arguments, against further disclosure and increased capitalization. In particular, it was argued that if the disclosure standards were adopted, those firms which leased extensively would be placed at a disadvantage in the capital markets relative to those which did not. If retroactive capitalization were required, as in SFAS 13, some lessees would find themselves in technical violation of loan indentures. And, at a second order level, progress toward a number of economy-wide goals would be hindered.

To the extent that these fears were warranted, one would expect, other things equal, to see a downward adjustment in lessee share prices during the 1973-1976 period, reflecting the present value of

new costs associated with the accounting standards in question. The relative behavior of lessee share prices during this period has been examined in four major studies. Two of the studies analyze the share price impact of the APB 31/ASR 147 disclosure rules; the others focus almost exclusively on SFAS 13. These studies, which differ substantially as to hypotheses tested, research design, and reported results, are reviewed below. An assessment is then made of the conclusions that can reasonably be drawn from the four studies taken jointly, and a discussion of unresolved issues is provided.

The first systematic study of the effect of a lessee accounting pronouncement on share prices was undertaken by Ro (1978), who hypothesized that the lease disclosures mandated by the SEC in ASR 147 would influence investors' perceptions of the risk-return characteristics of, and therefore the share prices of, affected firms. This proposition was tested using a sample of 99 pairs of lessee and nonlessee firms, matched on the basis of systematic risk and (where possible) SIC industry code. Ro examined mean monthly return differences from January 1973 (six months prior to the SEC's initial proposal of ASR 147) to September 1974 (six months after the first ASR 147 disclosures). Several overlapping subperiods within the primary study period were also investigated. Finally, mean return differences for calendar 1972 were examined in order to determine the adequacy of the matching procedure.

Ro performed statistical tests on mean return differences for the entire matched sample and for various subsamples, reporting the following results:

- a. During the 1972 (nonstudy) period, mean return differences were essentially zero.
- b. During the primary test period and during all subperiods but one, lessee returns were significantly lower than those of the matched non-lessee firms.
- c. The pattern indicated in (b) was apparently attributable to the subsample of firms ("PV-IE firms") which responded to ASF 147 by reporting both the "present value of noncapitalized financing leases" and a material pro forma income effect. Mean return differences were generally insignificant for the group of firms ("PV" firms) which did not report a material income effect.
- d. Return differences of high- β firms were on average more pronounced than those of low- β firms.¹²

Based on these results, Ro concluded that "the SEC's extended lease disclosure decision of 1973 had an impact on the pricing of securities for the firms which were affected by the decision" (Ro, 1978, p. 336). To explain the markedly different share price behavior of the "PV" and "PV-IE" firms, he reasoned that either "the balance sheet effect itself of capitalized lease disclosure . . . may not be as great as is generally believed," or alternatively, that "the balance sheet effect (if any) was already impounded in security prices prior to the SEC lease decision" (p. 336).

Ro's study was extended by May, Harkins, and Rice (MHR, 1978), who suggested that if the disclosures mandated by the APB and SEC affected the market's assessment of the risk-return characteristics of lessee firms, then at least one of two possible types of price behavior should be observed. First, if estimates of the extent of firms' leasing activities were for some reason systematically biased -- perhaps due to the costliness of obtaining leasing information from non-accounting sources -- then one should observe a change in the

level of lessee share prices relative to those of non-lessees. Second, even in the absence of such a systematic bias, lessee shares should exhibit greater than normal price variation as new information about specific firms is "impounded" by the market. MHR also argued that, at the firm level, the degree of price response should vary directly with the degree of change in the level of the firm's disclosures and with the materiality of the firm's lease obligations. Finally, the authors pointed out that the timing of the observed price response would depend on the extent to which speculative information acquisition and trading occurred during the period preceding the first disclosures under the new standards.

MHR investigated these hypotheses by examining 1972-74 10K and annual reports for a random sample of 314 firms. "Degree of change in the level of lease disclosure" was measured qualitatively by reference to pre- and post-APB 31/ASR 147 disclosures. In addition, a rental coverage ratio was computed for each firm as an indicator of the materiality of lease commitments. The total sample was partitioned into subsamples representing combinations of materiality level and level of change in disclosure practice, and an average cumulated residual return (as per Fama and MacBeth (1973)) was computed for each subsample over the period February 1970-December 1974.

MHR's extensive analysis of average cumulated residuals produced, among others, the following results:

- a. Over the period studied, lessee share prices declined significantly relative to those of non-lessees.

- b. The magnitude of relative decline was found to be strongly positively correlated with the materiality measure. Moreover, the materiality of lease commitments appeared to account for both "PV/PV-IE" effect and, to a lesser extent, the risk dependence of mean return differences observed by Ro.
- c. The most dramatic relative price declines were exhibited by the upper 40%, materiality-wise, of those firms which had previously disclosed some quantitative information about lease commitments. In February or March 1972, these shares entered a lengthy period of steady decline which persisted on average until June 1973, the month in which APB 31 was adopted.
- d. Little evidence of significant abnormal price behavior was found around the date of the first APB 31/ASR 147 disclosures.

Subject to numerous caveats, the authors appear to interpret their evidence as being consistent with a systematic "market under-assessment," prior to the beginning of lessee accounting deliberations by the APB, of the extent of the leasing activities of those firms for which leasing was "very material." They suggest that speculative information production and trading during the deliberation period had the effect of "unbiasing" estimates of leasing activity, and that this "unbiasing" was largely accomplished prior to the time at which the new disclosures first became available.

The Ro and MHR studies of share price response to the 1973 APB/SEC disclosure rules assume that the imposition of these rules represented a pure "information event." In the two studies of SFAS 13 share price response to be reviewed below, such information effects are de-emphasized. The general argument made by the authors of these latter studies is that any abnormal price response to SFAS 13 must be

attributed to the standard's indirect effect on the production, investment, or financing decisions of the firm.

Abdel-khalik, Ajinkya and McKeown (AAM, 1981) analyzed common stock returns for groups of lessee and non-lessee firms over periods of 13-22 weeks surrounding each of five standard setting events: the issuance of the first and second exposure drafts, the adoption of SFAS 13, the issuance of ASR 225,¹³ and the period during which most firms applied SFAS 13 retroactively. The lessee sample included all NYSE and AMEX airline, fast food, and retail firms; and from other industries, all firms for which the present value of non-capitalized financing leases (per ASR 147) exceeded both 25% of book long term debt and 5% of book long term capitalization as of the end of fiscal 1975. Non-lessee control firms were randomly selected from the remainder of the NYSE/AMEX population.

AAM argued that a firm's production-investment-financing decisions would be affected by SFAS 13 only if (a) non-capitalized leases represented a large component of capital structure, and (b) the firm lacked the necessary flexibility to mitigate the financial statement effects of the standard. The ratio of the present value of noncapitalized financing leases to book value of long term debt, and firm size as measured by the book value of total assets, were adopted as proxies for these two characteristics. The lessee sample was thus cross-partitioned by industry type, size, and the materiality of SFAS 13's effect on financial statistics. Control samples were partitioned on the basis of firm size only. The expectation implicitly expressed by the authors was that if lessee share price behavior differed during

the SFAS 13 deliberation period from that of similarly sized non-lessees, the observed differences would be most pronounced in the case of small firms with substantial lease financing.

AAM examined these arguments empirically by computing average cumulative standardized market model residuals for each lessee and non-lessee subsample over each of the "event periods" studied. The residuals were tested for statistical significance with the following results:

- a. During the first exposure draft period, lessee firms generally exhibited a strong pattern of significantly positive residuals.
- b. During the second exposure draft period, lessee firms generally exhibited a strong pattern of significantly negative residuals.
- c. The behavior of lessee residuals during the other periods studied was either non-significant or industry specific.
- d. During the first and second exposure draft periods, lessee and non-lessee residuals exhibited qualitatively similar behavior. Moreover, in the case of lessee firms the anticipated pattern of residuals according to firm size and materiality of lease commitments was not observed.

Based on these results, and most specifically on result (d), AAM concluded that while significant shifts of relative share prices occurred during the periods studied, these shifts did not appear to be related to the SFAS 13 standard setting process.

The opposite general conclusion is drawn by Pfeiffer (1980) from the results of a study which is in many ways similar to that of AAM. Pfeiffer suggested that SFAS 13 could have been expected to have a negative impact on lessee share prices due either to its effect on the firm's position with respect to loan covenants, or to its effect on

the accounting numbers which, in many cases, are used in determining the level of management incentive compensation. In this study, the price behavior of 260 lessee firms was examined for periods of 11-18 weeks surrounding each of the following events: the adoption of APB 31, the adoption of ASR 147, the issuance of the discussion memorandum on lease accounting by the FASB, the FASB public hearing period, the issuance of the first and second exposure drafts, and the final adoption of SFAS 13. "Lessee firms" were those for which capital leases exceeded 5% of total assets, apparently at the end of fiscal 1976. A control sample of non-lessee firms was also constructed for use in some parts of the study.

Pfeiffer's analysis of returns took on two forms. The first was a series of portfolio return comparisons, in which portfolios were formed to reflect either firm characteristics (e.g., lessee v. non-lessee, management incentive plans present or absent) or aspects of SFAS 13 financial statement impact (e.g., large v. small effect on income, retained earnings, book leverage, working capital). Under the assumption that returns are generated by a multifactor process, and the further assumption that a firm's sensitivity to various factors can be indexed by a set of market- and accounting-based "descriptors," individual securities were weighted to form portfolios which were equivalent in terms of systematic risk, sensitivity to short term interest rate fluctuations, dividend yield, book leverage, size, and current position.¹⁴ To the extent that this weighting scheme controls for all factors affecting returns in general, the likelihood is greatly increased that an observed difference in portfolio returns is

in some way related to the dimension on which the portfolios are differentiated. Pfeiffer's portfolio return comparisons produced the following results:

- a. Returns to the shares of lessee firms were significantly lower (dramatically so, considering the short time periods involved) than returns to non-lessee shares during the "public hearing" and "second exposure draft" periods.
- b. During the same periods, lessee firms for which the financial statement impact of SFAS 13 was "large" exhibited significantly lower returns than did lessee firms for which the financial statement impact was "small."
- c. A small number of firms which were known to have renegotiated loan contracts as a result of SFAS 13 exhibited relatively low returns during the "public hearing" period.
- d. During other periods, and for other dimensions used as a basis for portfolio formation, portfolio returns did not in general differ significantly.

In the second phase of Pfeiffer's analysis, which focused only on the public hearing period, lessee mean returns were regressed cross-sectionally, first on a market-based estimate of systematic risk, and then on an extended set of variables which also included four measures of the impact of SFAS 13 on financial statement numbers and relationships. As might be expected from the portfolio comparison results, the extended regression model "explained" a significantly greater proportion of mean return variation than did the model based solely on systematic risk.

Based on the various empirical results described above, Pfeiffer concluded that "SFAS 13 had a significant negative impact on stock returns of lessee firms," and observed that "the magnitude of the

effect appears to be related to the effect on lending agreements" (Pfeiffer, 1980, pp. 41-42).

Ignoring for the moment the various interpretations and conclusions expressed by the authors, the broad empirical results of these studies can now be summarized. Three of the studies reviewed -- Ro, MHR, and Pfeiffer -- indicate that after some attempt has been made to control for the effects of factors which influence security returns in general,

- (1) the shares of lessee firms appear to earn lower returns than those of non-lessee firms during a lengthy period surrounding the adoption of APB 31 and ASR 147, and also during shorter periods surrounding at least some of the events which led to the adoption of SFAS 13; and
- (2) the magnitude of the observed difference appears to be positively associated with the "materiality" of a firm's lease commitments (as measured, for instance, by Ro's distinction between "PV" and "PV-IE" firms, by MHR's rental coverage ratio, or by the financial statement impact indices used by Pfeiffer as a basis for portfolio formation).

The results of the fourth study are difficult to compare to those of the first three for several reasons. First, AAM present only qualitative information concerning the return behavior of their various portfolios. The results of statistical tests on portfolio return differences are not reported. Second, while the natural comparison to make is AAM v. Pfeiffer, the most consistently negative lessee share price behavior observed in the latter study occurred during the "public hearing" period. This period was not examined by AAM. Third, it is possible that the "materiality" measure employed by AAM as a basis for portfolio formation -- the ratio of noncapitalized financing leases per ASR 147 to long term debt -- is not, whatever its

other merits, very highly correlated with those used in the other studies. For example, it is easy to envision situations in which a low level of leasing activity could be quite large in relation to long term debt and yet immaterial in terms of its impact on other financial statistics. Because of this lack of comparability, the discussion to follow will be based mainly on the results of the first three studies.

Given these results, two questions of interest can now be addressed. First, can it be concluded with any large measure of confidence that lessee share prices were actually affected by the standard setting activities of the APB, SEC, or FASB? And second, if one assumes that such a conclusion is warranted, to what extent does the available evidence allow one to distinguish between possible reasons for such an effect?

The most general answer to the first question, as the authors of all studies reviewed here have pointed out, is that ex post empirical studies do not prove causal relationships, since it is technically impossible to control for all competing explanations of a particular phenomenon. Speaking in more practical terms, however, if all competing explanations which are more than "remotely probable" can be eliminated via the use of appropriate controls, cause and effect interpretations can be stated with greater confidence than would otherwise have been the case. Thus, faced with evidence which is consistent with the hypothesis that lessee share prices were affected by the imposition of lessee accounting standards, what needs to be assessed is the extent to which more than remotely probable competing

explanations have been satisfactorily excluded by the control procedures employed in these studies.

The set of more than remotely probable competing explanations for such evidence includes any simultaneous event, or series of events, whose share price impact would probably have been systematically different for lessee and non-lessee firms, or for "material" and "non-material" lessees. One competing explanation, of potential importance in any study which examines contemporaneous security returns, is that if lessee and non-lessee firms tend to respond differently to general economic events, the use of an incorrectly specified model of conditional expected returns could conceivably be responsible for the observed pattern of empirical results. This alternative deserves serious consideration, particularly in light of the fact that the period examined in these studies was characterized both by dramatic changes in the general level of share prices and by greater than usual return volatility.

Each study reviewed here deals similarly with the problem of simultaneous events. It is first assumed that the j^{th} security's realized return over a given period can be decomposed into three components, as follows:

$$\tilde{R}_{jt} = E_j + f_j(\tilde{\delta}_{1t}, \dots, \tilde{\delta}_{kt}) + \tilde{u}_{jt} . \quad (1)$$

In (1), E_j is an unconditional expected return, $f_j(.)$ is a function of k random economic factors which affect all security returns (the intensity of the effect depending on a particular security's characteristics), and $\tilde{u}_{jt}$ reflects the net impact of all

security-specific events, including the effect (if any) of the lessee accounting event of interest. Because the lessee/non-lessee distinction could conceivably proxy for inter-security differences in any or all of these components, a specific empirical model is selected to represent (1), and model characteristics (i.e., E_j and f_j) are estimated for each security to be studied. These estimates are used to control for systematic lessee/non-lessee return differences which could have been expected, given the general pattern of economic events during the period studied. In effect, such a procedure allows attention to be focused directly on the behavior of the u_{jt} .

Obviously, this procedure can produce spurious results if the empirical model chosen to represent (1) is incorrectly specified. One plausible source of such misspecification might be referred to as the "missing factor" problem. Suppose that the correct model for R_{jt} depends on k random economic factors, of which only $(k-1)$ are controlled for successfully by the assumed model. In addition, suppose that returns to lessee and non-lessee shares tend -- for any reason -- to differ in their sensitivities to the behavior of the k^{th} factor. Under these conditions, it would hardly be surprising to observe a significant difference between lessee and non-lessee returns for a given period, even after controlling for the effects of the $(k-1)$ factors recognized in the assumed model.

In the Ro, MHR, and AAM studies, conclusions are based on conditional prediction errors which were calculated using either a one factor "market" model or on a time series analogue of the traditional capital asset pricing model. As an empirical matter, it is well known

that prediction errors associated with these versions of (1) are not independent across securities, which indicates that an additional factor or factors must be taken into account in order to explain the systematic behavior of returns.¹⁵ Thus, one can interpret the results of these studies as being consistent with an "accounting standard effect," a "missing factor effect," or both.

Pfeiffer recognized and nominally dealt with this problem by representing (1) as a k factor linear model:

$$\tilde{R}_{jt} = E_j + b_{jt} \tilde{\delta}_{1t} + \dots + b_{jk} \tilde{\delta}_{kt} + \tilde{u}_{jt} . \quad (2)$$

This is the returns generating process on which the Ross (1977) arbitrage pricing model is based. Actually, Pfeiffer left k unspecified and assumed that each of the coefficients $b_{j1}, \dots, b_{jk}$ could in turn be represented as a linear combination of certain measureable or estimable characteristics of security j. As pointed out previously, these characteristics included j's "market β ," the sensitivity of its return to short term interest rate changes, its dividend yield, and the size, leverage, and current position of the underlying firm. To the extent that this assumption is correct, controlling for these characteristics would also control for any systematic differences between lessee and non-lessee returns. However, the appropriateness of this control procedure was never directly tested, and the degree to which it was successful in eliminating systematic return differences is unknown.

It should be pointed out in passing that even if Pfeiffer's procedure were capable in principle of perfectly controlling for

systematic lessee/non-lessee return differences, it is still possible that his use of accounting numbers to proxy for certain firm characteristics resulted in "matched" lessee and control samples which were actually fundamentally different. One example can be provided to illustrate this point. Pfeiffer in essence computed the following measure for each lessee and non-lessee firm:

$$LEV_j = (D_j/A_j)/M(D/A) ; \quad (3)$$

D_j and A_j are the book values of firm j 's total debt and total assets, respectively, and $M(D/A)$ is the mean value of this ratio for all Compustat firms. In order to control for financial leverage when comparing returns to portfolios of lessee and non-lessee firms, each portfolio was weighted to satisfy the requirements

$$\sum w_j LEV_j = 1 , \quad (4a)$$

$$\sum w_j = 1 , \quad (4b)$$

$$\text{and } w_j > 0 \text{ for all } j, \quad (4c)$$

where $[w]$ is a vector of portfolio proportions. That is, the "leverage characteristic" of each portfolio was set to equal a market wide average. However, because end of fiscal 1972 book values were used as a basis for these measures, the leasing component of financial leverage was effectively ignored. If the leverage measure is redefined to include the present value of non-capitalized leases, i.e.

$$LEV_j = \frac{D_j + NCL_j}{A_j + NCL_j} \div M \left[\frac{D + NCL}{A + NCL} \right], \quad (5)$$

it can easily be shown that portfolio weights satisfying (4) will produce a lessee portfolio whose financial leverage is strictly greater than that of the related non-lessee portfolio. If, as Pfeiffer assumed, leverage is an important argument in the "systematic" portion of (2), the fact that his lessee and non-lessee portfolios are likely to have differed on this dimension provides an alternative explanation for the observed return differences. A similar argument can be made with respect to the measurement of firm size, for which the book value of total assets was taken as a proxy.

To summarize, because of a potential "missing factor" problem, lessee/non-lessee return differences which have tentatively been attributed (in the studies reviewed above) to the impact of lessee accounting standards may simply reflect -- to some extent -- systematic differences in the return behaviors of lessee and non-lessee shares.

In addition to the "missing factor" problem, a second plausible source of model misspecification, which will be referred to here as the "nonstationarity" problem, could also account for some or all of the lessee/non-lessee return differences observed in these studies. This problem arises if the parameters (E_j and f_j) of (1) are nonstationary, if their direction of change and rate of change are in part the result of an interaction between security-specific characteristics and aggregate economic phenomena, and if the lessee/

non-lessee distinction proxies for one or more of the relevant security-specific characteristics.

Many studies of stock price behavior indicate that the market model slope parameter (β) is nonstationary.¹⁶ And in the theoretical realm, a number of studies suggest that changes in leverage occasioned by drifts in firm value may be an important source of such nonstationarity. For example, Galai and Masulis (1976) show that, if the equity of a levered firm which pays no dividends is regarded as a European call option on the firm's assets, then

$$\beta_s = \left(\frac{\partial S}{\partial V} \cdot \frac{V}{S} \right) \beta_v = \eta_s \beta_v, \quad (6)$$

where β_s and β_v are instantaneous values (for the firm's equity and assets) of the slope parameter of the market model version of (1), and η_s represents the instantaneous elasticity of equity value (S) with respect to firm value (V). For a given level of debt, η_s is shown to be a convex and monotonically decreasing function of firm value, approaching unity as V becomes large.¹⁷ Thus, if β_v is presumed to be stationary, β_s should increase as V falls, and vice versa. Moreover, the convexity of η_s indicates that if firms i and j differ only by a scale factor and by degree of leverage -- i.e., if $V_i/S_i > V_j/S_j$, then β_{si} should be more volatile than β_{sj} with respect to changes in V . These results suggest (but do not absolutely imply) that during a period of steady decline in market values, the market model slope parameter will increase for all levered equities, and that the rate of increase will be faster for highly levered firms.¹⁸ Likewise, during a period in which market values steadily increase, the equity slope

parameter will generally decrease, and again, the rate of decrease will be faster for highly levered firms.

The results of the studies reviewed above can be interpreted in the context of this scenario. Ro, MHR, and Pfeiffer estimated market model β 's over a period preceding APB 31 and ASR 147. These estimates were used in turn to compute conditional prediction errors (i.e., estimates of u_{jt} in (1)) during subsequent periods of three months or more surrounding various standard-setting events. The fact that average prediction errors for lessee firms were more negative than those for non-lessees over some of the periods studied is the primary evidence supporting the "adverse share price effect" hypothesis.

Evidence will be presented in Chapter 5, however, to indicate that on average, lessee firms are more highly levered than non-lessee firms. That is, the lessee/non-lessee distinction proxies for "degree of leverage." In addition, an examination of various stock market indices shows that equity values (and, presumably, firm values) declined steadily and dramatically throughout 1973 and 1974, the period during which most of the results of the above studies were observed.¹⁹ These facts, together with the nonstationarity scenario just outlined, suggest that some proportion of those results can be explained in terms of differential biases in the measurement of u_{jt} . In particular, if "true" equity β 's are increasing relative to their estimated values, the rate of increase being faster for lessees than for non-lessees, and if market returns are on average negative, then a downward bias will be imparted to the difference between lessee and non-lessee average prediction errors.

Figure 2.1, a plot of cumulative average lessee/non-lessee prediction error differences over the period 1973-1978 (inclusive), lends some credibility to the notion that the Ro, MHR, and Pfeiffer results may be due in part to "nonstationarity" problems. As was done in those studies, the values of $\hat{u}_{jt}$ used to construct this plot were computed using pre-1973 β estimates.²⁰ The negative drift of the curve over the 1973-1974 declining market period is consistent with earlier findings. However, the direction of the drift is reversed during the generally rising market of 1975-1978, and such a reversal is precisely what one would expect under the nonstationarity scenario described above. That is, if "true" equity β 's are larger (relative to their estimated values) for lessees than for non-lessees, and if market returns are on average positive, then an upward bias will be imparted to the difference between lessee and non-lessee average prediction errors.

The preceding discussion can now be summarized. Three of the four studies reviewed above report empirical results which are consistent with the hypothesis that lessee share prices were adversely affected by the various lessee accounting standards adopted during the mid-1970's. However, because these results are derived from an examination of contemporaneous security returns, it is argued that failure to adequately control for systematic return differences, due either to the "missing factor" problem, to the "nonstationarity" problem, or to biases in the measurement of firm characteristics, constitutes a "more than remotely probable" alternative explanation of

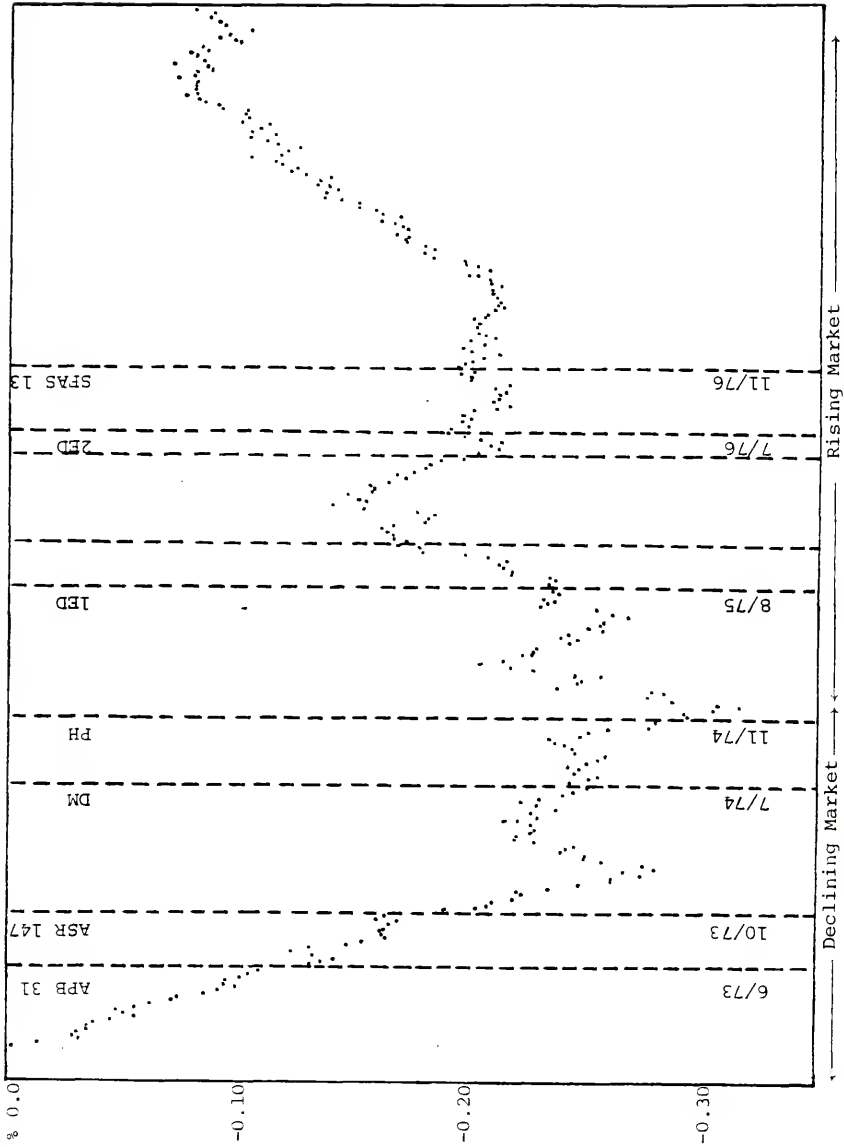


Figure 2-1
Cumulative Average Prediction Error Differences
Lessee and Non-Lessee Firms

the evidence. It is further argued that until this competing explanation can be satisfactorily eliminated, only limited confidence can be placed in the conclusions that a lessee accounting standard/lessee share price effect actually occurred.

In order to accomplish this, one must have in hand a fully developed (and fully validated) dynamic model of the "returns generating process" -- something that is not likely to be available in the foreseeable future. Thus, the discussion above is offered as an explication of a problem, rather than as a criticism of the various studies reviewed. The importance of the problem itself, however, cannot be overemphasized, particularly in an era in which policy making groups appear to be using studies of the type reviewed either as inputs to the policy decision process or as ex post validations of decisions already taken. While the measurement approach taken in the present study (to be described in Chapter 4) will not solve the problem, it is hoped that it will provide additional insights concerning the magnitudes of lessee accounting standard share price effects.

Although it is important to know whether lessee accounting standards -- or for that matter any accounting standards -- have had a significant impact on share prices, the value of this knowledge depends on the degree to which the impact can be explained. Thus, if it is temporarily assumed that the empirical results reviewed above are indicative of a share price impact, the second of the two questions posed earlier must still be addressed: to what extent does

the available evidence allow one to distinguish between possible reasons for such an effect?

Two broad hypotheses can be discerned in the studies described above, both of which have frequently been reflected in the comments submitted by respondents to various lessee accounting proposals. These hypotheses can briefly be stated as follows:

- H₁: Information whose production cost previously exceeded any related benefit becomes available to the market as a result of the adoption of a lessee accounting standard. This information affects share prices because it alters investors' perceptions of the risk/return characteristics of lessee securities.
- H₂: Certain contract terms, which are expressed with reference to financial accounting numbers, and whose negotiation was conditioned on established financial accounting practice for lessees, are effectively altered due to the adoption of a new lessee accounting standard. Share prices are affected because the alteration in contract terms narrows the set of wealth maximizing opportunities available to shareholders.

Throughout the remainder of this study, H₁ and H₂ will be referred to as the "information" and "contracting" hypotheses, respectively. As indicated in Chapter 1, a primary goal of the present study is to determine whether the cross-sectional pattern of (apparent) lessee accounting standard-related share price effects is consistent with a particular aspect of the contracting hypothesis: namely, that the price impact is "more adverse" in the case of firms having substantial loan contracts whose terms are stated with reference to financial accounting numbers and relationships.

While the footnote disclosures required by APB 31 and ASR 147 may have resulted in the provision of new information, it is unlikely that

these standards had any direct effect on contract terms. At first glance, then, it would seem that the information hypothesis, and only the information hypothesis, is relevant in the context of the Ro and MHR studies. On the other hand, SFAS 13 can be viewed as a standard whose principal effect was to require that these disclosures be incorporated in the firm's financial accounts, thus potentially affecting contract terms but providing little in the way of new information. This characterization of SFAS 13 suggests that the contracting hypothesis, and only the contracting hypothesis, is relevant in the context of the Pfeiffer and AAM studies. It is possible, however, to interpret all the evidence which links lessee accounting pronouncements with negative share price behavior as being consistent with either hypothesis, or with some combination of the two, depending on what assumptions one makes about the mechanism by which information is impounded in prices.

For example, Ohlson (1979) assumes a market composed of rational, wealth maximizing investors having homogenous beliefs based on identical access to information. In this setting, he considers the effect of a change in the disclosure environment (e.g., a shift to a finer set of disclosures regarding leasing activities). A basic result of his analysis is that, if the change has no impact on firms' production-investment-financing programs, the average price effect of the change (over a large number of securities) should be essentially zero. This is because pre-change security prices will have been based on unbiased estimates of all price-relevant security characteristics.²¹ If it is believed that "real" market behavior approximates

that which would obtain under these idealized conditions, it becomes difficult to reconcile the Ro and MHR results (and virtually impossible to reconcile the Pfeiffer results) with the information hypothesis. However, if one interprets the various events leading to the adoption of APB 31, ASR 147, and SFAS 13 as signalling increases in the probability that lease capitalization would ultimately be mandated, then the contracting hypothesis -- in which an impact on production-investment-financing programs is explicit -- provides an appealing explanation of the observed results. In particular, the association between the magnitude of share price decline and the materiality of leasing activities makes sense under the contracting hypothesis since, other things equal, the more material a firm's leasing activities, the greater the potential impact on contract terms.

The characteristics of Ohlson's market represent one extreme of a spectrum of possible characteristics. At the other end of this spectrum, one might envision a market composed of investors who differ widely in access to information, in information processing capabilities, and therefore in beliefs about the price relevant attributes of each security. If these differences were sufficiently pronounced, security prices could depend on the disclosure environment in a systematic way. A change in disclosure policy might then create conditions under which, for example, investor class A could profit at the expense of investor class B by adopting trading strategies which anticipate the effects of the change on B's beliefs.²² In such a market, the information hypothesis could conceivably explain the

results obtained in the various studies described here, even if contracting effects were totally absent.

If the market is indeed "segmented" in the manner just described, however, it is difficult to make other than a non-directional hypothesis concerning share price impact. In order to go much further than this, one would require a fairly detailed specification of the actual structure of the market. Since the focus of the present study is upon the extent to which the observed pattern of share price movements is consistent with that which could be expected under the contracting hypothesis, a market of the "Ohlson" type is assumed as a maintained hypothesis throughout the study. The immediate logical impact of this assumption is to rule out the information hypothesis as an explanation of observed share price movements. In practical terms, the ability to disregard the information hypothesis greatly simplifies matters of research design. Moreover, the assumption is not unreasonable, in that very few studies in the accounting and finance literatures have produced results which actually contradict the notion of an "Ohlson"-type market. In the final analysis, however, the making of such an assumption means that any conclusions regarding the aptness of the contracting hypothesis as an explanation of share price movements must be tempered by recognition of the possibility of simultaneous "pure information" effects.

2.5 Summary

The present chapter provides a description of the 1973-1976 lessee accounting standard-setting process, a discussion of the

financial statement impact of SFAS 13, the eventual output of that process, and a critical review of four empirical studies which have attempted to determine whether lessee equity prices were significantly influenced by the process. It is concluded that the results of the four studies, taken jointly, are at least consistent with the hypothesis of significant influence. It is also pointed out that the major focus of these studies is on whether, rather than why, such influence was exhibited. A primary intention of the present study is to examine the consistency of the data with one possible explanation of why share prices should be influenced by the adoption of accounting standards which appear, to all intents and purposes, to have been largely cosmetic. This explanation, referred to as the "contracting hypothesis," will be described in greater detail in the chapter which follows.

2.6 Notes

1. Using data from a source quoted in Bowman (1978) it can be calculated the annual revenues to the leasing industry (approximately \$10 billion in 1977) grew at a compound annual rate of about 25% over the period 1950-1977.
2. See Cary (1949), Gant (1959), Vancil and Anthony (1959), Dieter (1979).
3. The 1962 study by Myers was sponsored by the Research Division of the American Institute of Certified Public Accountants, and published as Accounting Research Study No. 4. Myers' broad conclusion is stated succinctly: "To the extent . . . that leases give rise to . . . property rights, those rights and related liabilities should be measured and incorporated in the balance sheet (Myers, 1962, p. 4). This conclusion was rejected by the APB in Opinion No. 5, apparently on the theory that a "property rights" approach to lease accounting would be inconsistent with accepted modes of accounting for other types of executory contracts.
4. According to the task force which prepared the 1974 FASB discussion memorandum on lease accounting, capitalization under Opinion No. 5 was in practice largely limited to those leases securing Industrial Development Bonds.
5. MHR presents the results of a detailed comparison of pre- and post-APB 31 lease disclosures for a random sample of 314 firms. An interesting aspect of these results is that, while the level of disclosure prior to APB 31 was low for the average lessee firm, those firms for which leasing activities were "most material" tended to disclose the most detailed supplementary data concerning leasing activities. Thus, it is not at all clear that such disclosures were generally inadequate prior to APB 31.
6. The "financing lease" distinction was required to be made if gross annual rental expense exceeded one percent of consolidated revenues. The pro forma income disclosure was required if either (a) the present value of noncapitalized financing leases equalled or exceeded five percent of the sum of long term debt, stockholders' equity, and the said present value, or if the impact of the capitalization on average net income for the most recent three years would have equalled or exceeded three percent.
7. Consider a lease in which the lessor retains title to the leased property upon termination. Holding constant (a) the ratio of residual value to the fair value of the property at the inception of the lease, and (b) the yield rate implicit in the lease, the substitution of an "investment recovery" condition for a "nominal

residual value" condition would make it more (less) likely that a relatively long term (relatively short term) lease would be capitalized.

8. A firm which makes all payments of principal and interest according to the schedule established in the loan agreement, but which violates one or more other covenants, is said to have committed a "technical violation." However, many such violations -- including violations of negative covenants involving accounting numbers -- are "events of default" in the legal sense. Thus, "technical violations" should not be regarded as mere "technicalities."
9. Of the 282 letters of comment received by the Board in response to the second exposure draft, 121 were direct submissions by lessee firms. The remaining 161 letters were submitted by accounting firms and organizations, various trade and professional associations, law firms, and lessors. Five lessee firms supported the second exposure draft generally and the retroactive capitalization provision in particular.
10. SFAS 13 was not the Board's final word on the subject of lease accounting. As of this writing, the Board has issued 7 amendments to and 6 "interpretations" of the standard. The SEC also (effectively) amended SFAS 13 by moving forward the required date for retroactive application (see ASR 225 and ASR 235, both issued during 1977).
11. See Dieter (1979), Dieter and Wyatt (1980), or any intermediate financial accounting text of current vintage.
12. In this context, " β " is the market model slope parameter which, in the capital asset pricing model of Sharpe (1964), is a measure of the riskiness of the related security or portfolio.
13. In ASR 225, the SEC required that SFAS 13 be implemented with full retroactive application in filings with the Commission subsequent to December 24, 1977.
14. This second assumption seems to be based, at least in part, on previous work by Rosenberg and Marathe (1975), who showed that estimates of systematic risk conditioned on a large number of such descriptors reflect subsequent return behavior more clearly than estimates based solely on the behavior of past returns. However, Pfeiffer provides no rationale for the particular set of descriptors used in his study.
15. See Roll and Ross (1980) for a review of the literature on this point.

16. See Sunder (1980) for a review and for empirical estimates of the step variance of β , calculated under the assumption that follows a random walk.
17. Black and Cox (1976) show that if the firm pays continuous dividends, and if the instantaneous dividend is determined as a constant proportion of firm value, then η_s will be decreasing "almost everywhere."
18. In order for this suggested behavior to unambiguously follow from the Galai and Masulis results, β must be stationary. Moreover, other implicit arguments of (6) -- for example, the riskless interest rate, the variance of asset returns, and the required level of payments to creditors -- must remain constant.
19. For example, by December 1974, the NYSE Composite Index had fallen to approximately half of its January 1973 level.
20. At the firm level,

$$\hat{u}_{jt} = R_{jt} - \hat{\alpha}_j - \hat{\beta}_j R_{mt},$$

where R_{jt} and R_{mt} are the realized returns, in week t , on security j and on the CRSP equal-weighted portfolio, respectively. The ordinary least squares parameters $\hat{\alpha}_j$ and $\hat{\beta}_j$ were estimated using 50 alternate weeks of returns during the 100 weeks preceding the first trading week of 1973.

21. The prices of individual securities would of course be expected to fluctuate if the "disclosure environment" change truly alters the available information set. However, these fluctuations should be independent across securities.
22. The behavior of prices in markets of this type have been modelled by Goldman and Sosin (1979) and by May and Noreen (1981).

CHAPTER 3

THE CONTRACTING HYPOTHESIS

3.1 Introduction

The ownership structure of a corporate firm can be viewed as a configuration of contracts which allocates the risks and benefits of the firm's investments among various claimants. The value of a given claim depends not only on the characteristics of those investments, but also on the particular contract terms which determine the (state-contingent) payoff stream to which it is entitled. Each class of claimants, motivated by economic self-interest, can be presumed to have adopted wealth maximizing strategies with respect to the contract terms by which it is affected. Thus, a potential for wealth transfer among claimholders is created when existing contract terms are perturbed through the action of some external agency. Moreover, the value of the firm as a whole should decline if transactions costs are incurred either in renegotiating affected contracts to eliminate, or in modifying production-investment-financing programs in order to adapt to, the effects of the perturbation.

Given this view of the firm, a mandated change in accounting method can affect security values by operating on contract terms which are stated with reference to accounting numbers measured in accordance

with generally accepted accounting principles. This is the essence of the contracting hypothesis. As was seen in Chapter 2, lessee opposition to lease capitalization and to retroactive application was routinely supported by arguments which were simply variations on this theme. Partly for this reason, but more generally because it offers a plausible explanation for an adverse share price reaction to an apparently "cosmetic" change in financial accounting policy, the contracting hypothesis as it relates to loan contracts is a major focus of the present study. Its various ramifications are discussed in detail in the remainder of this chapter. Section 3.2 is devoted to a review of the basic existing empirical study of loan contract-related share price effects. Section 3.3 provides some general information concerning the use of accounting constraints in loan contracts, together with an example of the effect of retroactive lease capitalization on the firm's position with respect to such constraints. In section 3.4, a model is developed of the cross-sectional distribution of returns arising from the adoption of an "adverse" accounting change. This model is summarized as a series of testable hypotheses in section 3.5.

3.2 The Leftwich Study

The first empirical investigation of contracting share price effects is provided by Leftwich (1981), who noted that an accounting standard which reduces the permissible set of financial accounting techniques also narrows the opportunity set faced by shareholders who are party to "accounting-based" contracts. The adoption of such a

standard should therefore decrease the aggregate value of shareholders' claims by an amount which, as discussed above, includes both wealth transfer and transactions cost components.

Leftwich's study focused on loan contracts which include provisions stated in terms of accounting numbers. In addition to making the general argument that share prices are adversely affected by an "option reducing" accounting standard when such contracts are outstanding, he also reasoned that the adverse effect should be mitigated to the extent that the firm's debt is directly placed, callable, or convertible. If debt is directly (i.e., privately) placed, the number of creditors involved is small, and the direct costs of renegotiating the loan contract to eliminate the effects of the accounting rule change should be relatively low. In the case of debt which is publicly offered but callable, the adverse price impact of an accounting rule change is limited to the cost of refunding the debt. And finally, the wealth transfer potential associated with such a rule change is more limited when the value of a loan depends in part on the presence of a conversion privilege.

These ideas were explored through an examination of the behavior of stock returns surrounding "significant events" during the period leading to the adoption of APB Opinion No. 16 -- Accounting for Business Combinations (Accounting Principles Board, 1970). The intent of APB 16 was to limit the use of pooling of interests accounting, and to require the use of purchase accounting, in many merger situations. Leftwich argued that, relative to purchase accounting, pooling of interests accounting has a beneficial effect (from the shareholders'

perspective) on those financial statement numbers and relationships typically used in restrictive loan covenants. Thus, the adoption of an accounting standard which narrows the range of conditions under which pooling is permissible should have been economically harmful to the shareholders of "merger-prone" firms.

When averaged over a large sample of firms having some history of merger activity, market model prediction errors were found to differ significantly from zero during (two week) periods surrounding nine of twenty two APB 16-related "events." For each of these nine events, prediction errors were then regressed cross-sectionally on measures of publicly offered, privately placed, callable and convertible debt, and on a measure of firm size.¹ Generally, the regression results suggest that the magnitude of the adverse price response associated with APB 16 varied directly with the extent to which firms were debt financed and inversely with firm size.² Beyond this, it appears that the direct placement, callability, and convertibility hypotheses were not particularly supported by the data, leading Leftwich to conclude that "contracting cost theory provides, at best, an incomplete explanation of the observed abnormal performance of firms during the APB deliberations on business combinations" (Leftwich, 1981, p. 33).

Whether this conclusion is warranted or even meaningful is open to question. If a complete explanation is taken to be one which perfectly models the non-systematic component of security returns at a given point in time, or even one which accounts for a large proportion of the cross-sectional variation observed, then such a conclusion could have been stated with a high degree of confidence even in the

absence of the accompanying research. On the other hand, if completeness is defined to exist only when a particular set of predictions is fully supported by a particular set of results, then "incompleteness" may be inferred either because the theory itself is false, or because the experimental design is not sufficiently fine to allow detection of the predicted effects. The fact that the Leftwich results do not support the direct placement, callability, and convertibility predictions may be due to the latter problem in particular. For example, suppose that a number of firms are equally "bound" by accounting-based loan covenants, and that their financial statements would be identically affected by the provisions of APB 16. Also suppose that the firms are identically levered, where leverage is measured as the ratio of the book value of debt to the market value of equity. If contracting costs are indeed significant, one would then expect the data to support second-order predictions such as those concerning direct placement, callability, and convertibility. These rather strong conditions are clearly absent from the Leftwich study, however, and hence it is difficult to interpret the regression coefficients estimated therein as straightforward tests of the predictions in question. For this reason, it seems more useful to suggest that Leftwich's results provide tentative support for the hypothesis that contracting effects of detectable magnitude do occur, than to conclude that, as an explanation of abnormal performance, the contracting hypothesis is incomplete.

Having reached this assessment, it becomes appropriate to search for a context other than APB 16 in which the contracting hypothesis

may be further explored. It can be argued that, for a number of reasons, the lessee accounting standard developments of the 1973-1976 period provide such a context. First, it seems clear that for many firms, the impact of lease capitalization on financial statement numbers and relationships -- and therefore the potential impact on contract terms -- was far from trivial.³ Second, under the final version of SFAS 13, the financial statement impact of lease capitalization was intensified due to the retroactive application provision, a feature that was completely absent in the case of APB 16. Third, the standard setting process leading to SFAS 13 included a major policy reversal on the matter of prospective v. retroactive application, which suggests a similar reversal in share price behavior. And finally, because the financial statement impact of lease capitalization is more easily proxied than is the impact of a standard which precludes (to some uncertain degree) the future use of pooling of interests accounting, the "lessee accounting context" provides a better opportunity for predicting the cross-sectional incidence of contracting share price effects.

The aim of the remainder of this chapter, then, is to develop testable hypotheses concerning the impact of APB 31, ASR 147, and SFAS 13 on share prices, given the contracting hypothesis. As in the Leftwich study, the discussion will focus on loan contracts, and in fact it will be assumed that the effect of these standards on other classes of contracts having accounting-based terms -- e.g., certain incentive compensation plans for top management -- was negligible. A

general description of the use of accounting restrictions in loan agreements is presented in the section which follows.

3.3 Accounting-Based Loan Covenants and Lease Capitalization

Jensen and Meckling (1976) have suggested that restrictive loan covenants are negotiated by shareholders in order to control the natural conflict of their own interests with those of creditors, thereby reducing the agency costs associated with debt financing.⁴ If penalties for violation are sufficiently large, such covenants place bounds on the set of actions shareholders may take in transferring wealth from creditors to themselves. These actions could include (a) the payment of dividends which are financed either through a liquidation of existing investments or a reduction in planned investment; (b) issuance of additional debt whose seniority equals or exceeds that of existing obligations; or (c) alteration of the returns distribution of the firm's assets, either through asset substitution, manipulation of new investment, or merger. If the shareholder-creditor conflict of interests is imperfectly controlled through contract provisions, then rational creditors will rely on "price protection" to insure an expected return which is consistent with the level of risk accepted. And, once a loan contract has been negotiated, rational shareholders can be expected to adopt a set of investment and financing programs which maximize their own wealth, subject only to the constraints imposed by the contract.

A number of common loan covenants restrict shareholder activities without reference to accounting numbers -- for example, virtually all

debenture contracts include clauses prohibiting the mortgage or sale and leaseback of those assets owned by the firm at the time the debentures are sold. On the other hand, many standard types of loan covenants are explicitly based on financial accounting numbers. Some of these place absolute floor or ceiling limits on accounting numbers or on relationships among accounting numbers. Others permit shareholders to undertake actions potentially detrimental to creditors only if conditions are met which depend directly on such numbers or relationships. Although by no means an exhaustive list, three of the most frequently encountered types of accounting-based loan covenants are described here to illustrate ways in which accounting numbers are used to limit the activities of shareholders.

Dividend pool. Many loan indentures limit the sum of dividends and net amounts expended for share repurchase to a fraction of accounting earnings accumulated since the inception of the contract. Thus, the ability of the firm to distribute assets to shareholders depends on the level of its accounting earnings and on the acceptable range of accounting techniques for computing earnings.

Restrictions on merger activity and the sale of additional debt. Many indentures permit these activities only if, on a pro forma basis, the firm's "assets in place" provide some acceptable level of coverage for funded debt. Accounting numbers are normally used as proxies for this relationship. For example, mergers might be permitted only if, on a post-merger basis, the ratio of net tangible assets to funded debt exceeds some constant. When such restrictions are present, the

firm's ability to alter its asset or capital structures depends on accounting definitions of assets and liabilities.

Working capital maintenance. Frequently, an indenture will define a minimum level of working capital which must be maintained while the loan is outstanding. The firm's ability to meet this restriction depends on, among other things, accounting definitions of assets and liabilities and on acceptable methods of segregating these items into their current and noncurrent components.

A very detailed discussion of the use of accounting numbers in loan agreements may be found in Commentaries on Indentures (American Bar Foundation, 1971, hereafter Commentaries). Evidence from this source, from a number of private placement agreements reviewed in Leftwich (1980), and from a selection of "public" indentures reviewed by the present author, suggests that "generally accepted accounting principles" constitute the basic measurement framework for loan covenants which involve accounting numbers.⁶ That is, GAAP is binding on the parties except where modified through specific contract provisions. Moreover, the same sources of evidence suggest that the firm's position with respect to accounting-based loan conditions is generally determined on the basis of GAAP in existence as of the moment of calculation.⁷ It is for these reasons that a policy-making body such as the APB or the FASB may inadvertently alter the provisions of loan agreements by expanding or contracting the set of accounting techniques which are permissible for financial reporting purposes.

The lessee accounting standards of 1973-1976 reduced the set of accounting techniques available to lessees, and the impact of the reduction on accounting numbers and relationships frequently used in loan contracts was (from the shareholders' perspective) clearly adverse. For example, with respect to the typical loan covenants described above, retroactive capitalization of existing leases has the immediate effect of reducing working capital, the dividend pool, and the ratio of net tangible assets to funded debt. And, to the extent that tightened capitalization conditions are binding on future leases, a firm's rate of growth with respect to typical accounting-based loan restrictions will also be reduced.

The effect of retroactive capitalization can be clarified with a simple example. Assume that a lease capitalization requirement has been imposed, effective as of the end of Year 5, and consider a hypothetical firm having the pre-capitalization balance sheet shown below.

Pre-Capitalization Balance Sheet

- End of Year 5 -

Current assets	\$ 200,000	Current liabilities	\$ 75,000
Non-current		Debentures (non-	
assets (net)	<u>800,000</u>	current portion)	300,000
		Capital stock	200,000
Total assets	<u>\$1,000,000</u>	Retained earnings	
		- restricted as to	
		dividends	200,000
		- unrestricted	<u>225,000</u>
		Total equities	<u>\$1,000,000</u>

This firm pays dividends which increase moderately from year to year; the Year 5 dividend was \$45,000. The contract between debenture-holders and shareholders includes four accounting-based restrictions which are binding so long as the bonds are outstanding. First, working capital of at least \$100,000 is to be maintained at all times. Second, the current ratio must be maintained at a level which equals or exceeds 2.0. Third, dividends are limited in total to \$50,000 plus earnings accumulated subsequent to the contract date (resulting, by assumption, in "unrestricted" retained earnings of \$225,000). And fourth, both mergers and additional borrowing are prohibited if, as the result of such activities, the ratio of net tangible assets to funded debt would be less than 2.0. And finally, two non-accounting restrictions have been included in the agreement. The first of these places limits on "off-balance-sheet" financing: aggregate annual rentals under leasing arrangements are limited to \$50,000. Second, proceeds from the mortgage or premature disposal of existing fixed assets must be used to liquidate outstanding debentures.

At the beginning of Year 1, the firm in question leased equipment with a fair market value of \$200,000. On that date, the equipment had an expected life of ten years and negligible expected residual value. The noncancellable lease term is ten years, and a payment of \$32,549 is due to the lessor at the end of each year. The interest rate implicit in the lease is 10%. And at the end of the lease term, the firm may purchase the equipment from the lessor for an amount equal to its appraisal value on that date.

Until the present time (i.e., the end of Year 5), this lease has been accounted for by charging \$32,549 annually against revenues. Some simple calculations will show that, if the lease is capitalized retroactively at the end of Year 5, \$100,000 will be added to non-current assets (assuming straight line depreciation) and \$123,387 will be added to liabilities. Of the latter, \$20,210 and \$103,177 will be classified as current and non-current, respectively. And unrestricted retained earnings will decline by \$23,387, the amount by which the increase in liabilities exceeds the increase in assets. Using these numbers, the firm's pre- and post-capitalization position with respect to the loan restrictions described above can be constructed, as indicated below.

	<u>Pre</u>	<u>Post</u>
Current ratio	2.67	2.10
Working capital	\$125,000	\$104,790
Unrestricted retained earnings (URE)	\$225,000	\$201,613
URE/Current dividend	5.00	4.48
Net tangible assets/funded debt	3.08	2.49

It should be noted that this example, while hypothetical, is in no way extreme or "pathological." The set of accounting-based loan restrictions assumed is representative of those faced by a broad range of firms, and the assumed scope of leasing activities is not atypical. The example illustrates the point alleged earlier, that the effect of capitalization is, from the shareholders' point of view, unambiguously adverse along every accounting dimension of the loan agreement. If

the policy change from "rental" accounting to capitalization was largely unanticipated at the moment of contract negotiation -- that is, if the levels of the various accounting restrictions were set on the assumption that rental accounting would be used over the life of the contract -- then capitalization has the effect of "tightening" those restrictions.

Under the rationality assumptions which are characteristic of all economic analysis, one must presume that the shareholders' precapitalization position with respect to loan restrictions was optimal. In this context, an optimal position would consist of a contemplated set of future investment and financing activities which maximizes current shareholder wealth, given existing loan restrictions, penalties for violation, and the current state of the firm. Since a policy change to capitalization has the effect of tightening loan restrictions, it seems clear that an optimal pre-capitalization position must, ceteris paribus, be sub-optimal on a post-capitalization basis.

It was pointed out earlier that a number of Leftwich's hypotheses, while themselves technically consistent with the general notion of contracting share price effects, were apparently not supported by the APB 16 data. It was also argued that these results should not be interpreted as grounds for rejecting the contracting hypothesis, and that further investigation in a different context would be appropriate. Actually, it seems highly probable that such effects do in fact occur. The real questions at issue are those of magnitude and generality. Do circumstances exist in which the contracting costs imposed by a mandated accounting change amount to a

non-trivial proportion of equity value? And are these circumstances sufficiently pervasive to allow the related share price effects to be detected? What will be attempted here, in order to begin to answer these questions, is the development of a model of the share price impact of a mandated change in lessee accounting rules when accounting-based loan restrictions are in force. First, some recent results from the finance literature are used to provide a very basic characterization of a loan contract, and the role of accounting restrictions in such a contract is considered. This characterization, together with some severely simplifying assumptions, is used to propose a model of the cross-sectional distribution of share price effects resulting from the adoption of an adverse accounting change. Attention is then devoted to the effect of relaxing the initial simplifying assumptions. The end result of this process consists of a set of hypotheses which relate a small number of observable variables to the distribution of returns at the time of an accounting policy change.

In the discussion which follows, the phrases "share price impact," "share price effect," etc. are meant to be understood in relative terms. That is, the contracting share price effect due to a mandated change in accounting practice is defined as the ratio of any contract-related costs incurred by shareholders to the pre-change market value of equity. It is important that share price effects be defined in this way, first to permit comparisons across firms, and second because the empirical analysis in this study will focus on returns rather than on absolute price changes.

3.4 Factors Influencing the Cross-Sectional Distribution of "Contracting" Share Price Effects

Consider the following characterization of a loan contract. Shareholders borrow B dollars from creditors to be invested, along with S dollars of personal funds, in specific assets having a current market value $V = S + B$. Over the life of the contract, shareholders are permitted to receive continuous dividends amounting to δV per unit time. When the contract terminates at time T , creditors are to receive $\min(C, V_T)$ dollars, and shareholders are to receive $\max(0, V_T - C)$ dollars. C is a fixed amount. The (ex-dividend) terminal value of the firm's assets, V_T , is assumed to have a lognormal distribution, the parameters of which are known both to shareholders and to creditors.

If asset and financial markets are frictionless, and if it can be assumed that shareholders will not deviate from the investment/financing program specified in the contract, then the exact value of B -- the amount that creditors are willing to lend -- can be determined (see Black and Scholes (1973), Merton (1974), Galai and Masulis (1976)). Generally speaking, B is a function of V , C , δ , and σ^2 , the last of which is the variance of dV/V , the instantaneous rate of return on the firm's assets. In somewhat different terms, if one defines $B = CD$, so that D is the current value per dollar of promised payment to creditors, then

$$D = f(V, C, \delta, \sigma^2), \quad (1)$$

with

$$D_V > 0; D_C, D_\delta, D_{\sigma^2} < 0, \quad (2)$$

where subscripted values of D represent partial derivatives. Once C , δ , and σ^2 have been fixed, creditors and shareholders will "share" all subsequent changes in V , with $dB = CD_V dV$ and $dS = (1 - CD_V) dV = S_V dV$. This means that a unique level of B (and therefore, of S) is associated with each possible level of V . A representative plot of B and S against V is shown in Figure 3-1. For very large values of V , B will approach $R_f^{-1} C$ and S will approach $V - R_f^{-1} C$, where R_f^{-1} is the current price of a riskless dollar to be delivered at time T . In this region, D_V and S_V will be very close to zero and one, respectively. For very small values of V , both B and S will approach zero, and D_V and S_V will tend respectively toward $(1/C)$ and zero.

This model of a loan contract, which has frequently been presented in the finance literature, differs from a typical loan agreement in two important respects. First, and most fundamentally, the partial derivatives of D indicate that shareholders will have an incentive to deviate from θ^* , the promised investment/financing program, once the loan is in place. In particular, any action which preserves V and simultaneously increases, C , δ , or σ^2 will increase the wealth of shareholders at the expense of creditors. Realizing this, creditors will be unwilling to lend unless the promises of shareholders can be adequately enforced. Enforcement is a simple matter if the value and variance rate of the firm's assets, the amount promised to creditors, and the amount disbursed as dividends can be observed at each point in time. With this information, creditors can determine whether shareholders have complied with contract terms, and the contract can be made self-enforcing if a sufficiently large

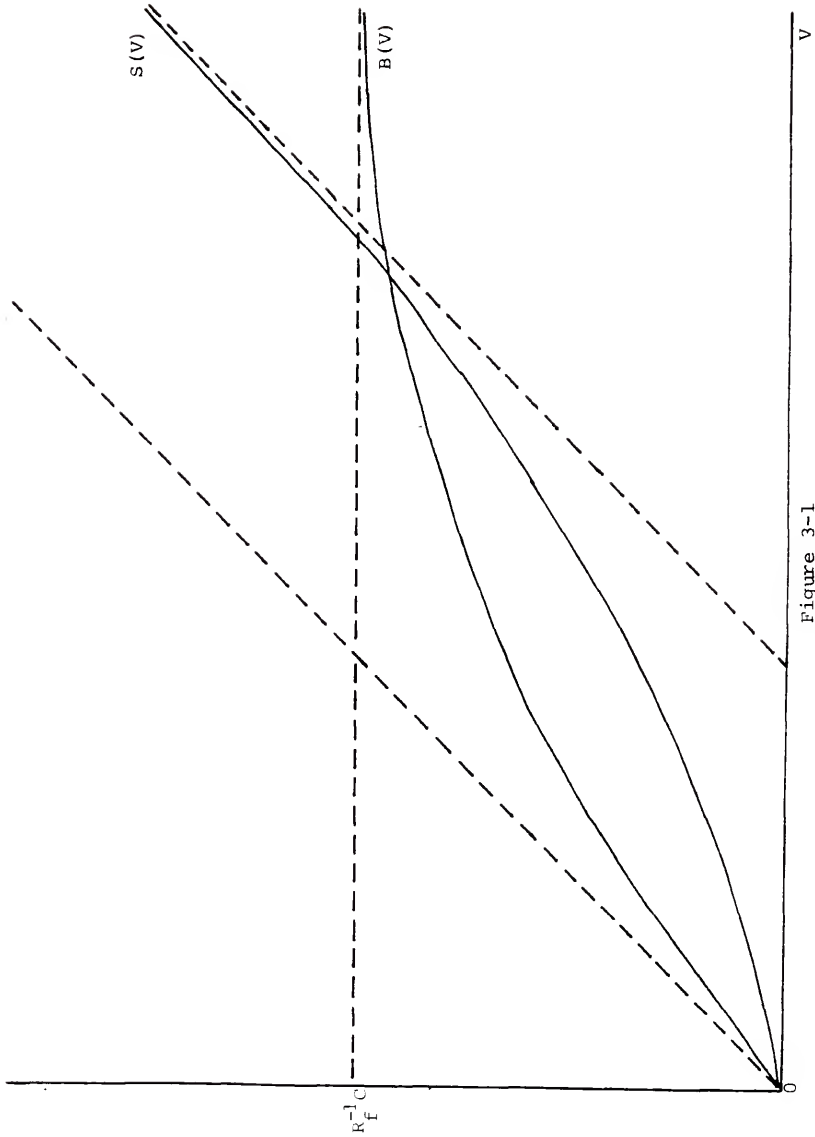


Figure 3-1
Typical Price Curves for Debt and Equity

penalty is imposed for non-compliance. In point of fact, however, neither V nor σ^2 -- nor, by extension, δ -- is directly observable. They are subjective quantities which must be appraised or assessed, and about which opinions are likely to differ where a conflict of interests exists. Thus, in a "real" contract, both shareholders' promises and the method of their enforcement must depend on something other than the theoretical variables which determine the value of the loan.

The second of the differences mentioned above arises because "fixing" the investment/financing program may not be efficient. If the firm's investment opportunity set changes over time -- for example, if new projects with positive net present value become available -- then it may be possible for shareholders to increase the value of equity without impairing the value of creditors' claims. In principle, this problem is easily remedied: shareholders simply promise not to depart from a given investment/financing program θ^* if, by so doing, creditors' interests would be adversely affected. In practice, of course, one is still faced with the question of how such a promise is to be enforced.

In order to make a real debt contract palatable to lenders, then, shareholders must include a system of promises (covenants) which can be monitored (preferably at low cost) by reference to observable variables, and which provide a reasonable degree of control over the wealth transfer problems associated with the partial derivatives of D : asset substitution, claim dilution, and excessive disinvestment. In addition, from the standpoint of efficiency, one would also expect the

system of covenants to allow shareholders some flexibility in the choice of investment/financing programs.⁸

In many cases, the system of covenants chosen to achieve these purposes is based largely on financial accounting numbers computed in accordance with (but sometimes allowing clearly specified departures from) generally accepted accounting principles. These numbers are observable and, due to the existence of a well organized auditing profession, can be monitored by reasonably independent outsiders at reasonably low cost. Moreover, it is apparent that relationships among accounting numbers are used to control or eliminate opportunities for "creditor to shareholder" wealth transfers. In the system of covenants described above in connection with the numerical leasing example, for instance, the fact that proceeds from the disposal of fixed assets must be used to liquidate existing loans means, in effect, that "asset substitutions" must be financed using new capital. The "claim dilution" problem is controlled by disallowing the sale of new debt if, as a consequence, total debt would exceed a specified proportion of tangible assets. The "excessive disinvestment" problem is controlled by establishing dividend limits which depend on the growth in net assets. Finally, it is equally apparent that systems of covenants can be designed which protect creditors' interests and which, at the same time, allow shareholders considerable latitude in the choice of investment/financing programs. Looking once more at the example just cited, it can be seen that on the investment side, no restriction is placed on the composition of the firm's asset portfolio. And on the financing side, only an overall constraint on

the relationship between tangible assets and funded debt need be met. Subject to this constraint, the financing arrangements for particular projects are unrestricted (except, as noted above, in the case of asset substitutions).

This discussion suggests the following characterization for a typical loan contract. Shareholders, contemplating a particular investment/financing program θ^* , promise to pay creditors $\min(C, \tilde{V}_T)$ at time T . In addition to this promise, the contract also includes a system of covenants based on observable variables, and a system of penalties which are of sufficient magnitude to guarantee shareholder compliance. Now, define ΔI as net new funds invested in the firm after the inception of the loan agreement, and D_V^* as the partial derivative associated with θ^* at the point $V^* = V - \Delta I$. The system of covenants is designed to have the approximate effect of insuring that

$$\frac{dD}{d\theta} \geq D_V^* \left(\frac{dV}{d\theta} - \frac{dI}{d\theta} \right) . \quad (3)$$

This contract has two desirable properties. First, the constraint established by the covenants means that, regardless of the manner in which θ is altered subsequent to the loan date, changes in firm value (other than those due to new investment) will be "shared" by creditors exactly as if θ has been fixed at θ^* . And second, the contract provides appropriate investment incentives for shareholders. Since $S = V - CD$,

$$\begin{aligned}\frac{dS}{d\theta} &= \frac{dV}{d\theta} - C \frac{dD}{d\theta} - D \frac{dC}{d\theta} \\ &= \left(\frac{dV}{d\theta} - \frac{dI}{d\theta} \right) - C \frac{dD}{d\theta} + \left(\frac{dI}{d\theta} - D \frac{dC}{d\theta} \right) .\end{aligned}\quad (4)$$

Substituting the constraint on $\frac{dD}{d\theta}$ and rearranging and collecting terms gives

$$\frac{dS}{d\theta} - \left(\frac{dI}{d\theta} - D \frac{dC}{d\theta} \right) \leq \left(\frac{dV}{d\theta} - \frac{dI}{d\theta} \right) (1 - CD_V^*) . \quad (5)$$

Wealth maximizing shareholders will require strict equality between the left and right hand sides of (5). The LHS can be interpreted as the change in the value of existing equity due to a small change $d\theta$ in the firm's investment/financing program. Shareholders will find $d\theta$ attractive only if the RHS of (5) is positive. If $d\theta$ is viewed as a "project" to be accepted or rejected, then the RHS is simply the product of the project's net present value and a positive number less than one. Thus, it will be in the best interests of shareholders to undertake projects which increase firm value and to reject those which do not.

The "bottom line" effect of this contract can be expressed in the following way. Regardless of the manner in which shareholders change θ subsequent to the contract date, the system of covenants guarantees (at least approximately) a minimum value per dollar of C at each possible level of $V - \Delta I$. If the actual value per dollar of C is allowed to fall below the required minimum, which can be denoted as D^* , severe penalties are imposed. On the other hand, if the actual value per dollar of C is allowed to rise above D^* , shareholder wealth

can be increased via further modifications of θ . Thus, if shareholders consistently adopt programs which maximize equity value, one would always expect to observe

$$S = V - CD^*, \quad (6)$$

where D^* is determined jointly by the level of $V-\Delta I$ and by the collection of covenants included in the contract.

One assumption explicit in this characterization of a loan contract is that both real assets and financial claims can be traded at zero cost. The addition of three further assumptions will make it possible to develop a very simple and unambiguous model of share price reaction in the face of an adverse accounting change. First, both within and across firms, all loan contracts outstanding are assumed to include equally restrictive accounting constraints. Second, it is assumed that no outstanding contract includes a call provision. And third, maximization of the value of a given loan is assumed to be consistent with maximization of creditor wealth.

Within this scenario, a mandated accounting change which tightens loan restrictions can be thought of as increasing D_V^* in (3) - or equivalently, as increasing D^* - at each level of $V-\Delta I$. Denoting the post-change required minimum for D as D^{**} , the "contracting" effect of the change on equity value can be written (in returns form) as

$$\frac{\Delta S}{S} = - \frac{C}{S} (D^{**} - D^*) . \quad (7)$$

In absolute terms, an adverse accounting change has the effect of transferring $C(D^{**}-D^*)$ dollars from shareholders to creditors. The

specific method by which this wealth transfer is accomplished will depend on the circumstances faced by a particular firm, but the set of available methods is indicated by the partial derivatives of D . Ceteris paribus, D can be increased by reducing the rate at which dividends are paid, by redeeming some portion of outstanding debt, by substituting "low variance" projects for existing or contemplated "high variance" projects, or by new infusions of equity capital. Another possibility is that the contract itself may be renegotiated to eliminate the effect of the accounting change, with compensation to creditors in the form of an increase in promised payment or an acceleration of loan maturity.

Expression (7) suggests a qualitative description of the cross-sectional distribution of $\Delta S/S$. First, if the impact of the change on those accounting numbers and relationships used to frame loan covenants is slight, then $(D^{**}-D^*)$ should be close to zero and $\Delta S/S$ should be small. Likewise, if amounts promised to creditors are small in relation to pre-change equity value, then C/S is close to zero and $\Delta S/S$ should again be small. If these conditions are individually sufficient for a "small" share price effect, their complements must be jointly necessary for a "large" share price effect. And since by assumption creditor wealth is maximized when the value of a given loan is maximized, their complements must be jointly sufficient for a "large" share price effect. More generally, at a given level of C/S (which can be interpreted as an index of leverage), the magnitude of $\Delta S/S$ should vary directly with $(D^{**}-D^*)$, and therefore with the

magnitude of the impact of the accounting change on financial statement numbers and relationships.

An implication of this description is that the contracting hypothesis can be tested using an experimental (or quasi-experimental) design which allows for a "leverage by financial statement impact" interaction. Before this idea is addressed, however, it must be admitted that the four assumptions involved above are hardly realistic, and that the proposed cross-sectional distribution of share price effects may not be appropriate once they are relaxed. Thus, some consideration should be given to the role of, and the effect of relaxing, each assumption.

The "zero transactions costs" assumption insures that none of the firm's resources need be expended in adapting to a mandated accounting change, and thus limits the shareholders' loss to an amount $w = -C(D^{**}-D^*)$. As indicated in Figure 3-2, an adverse accounting change can be thought of as increasing D^* (and therefore the value of debt, $B = CD^*$) at each level of V , in effect shifting the curves which relate B and S to V upward and downward, respectively, to B' and S' , with

$$B'(V) - B(V) = -(S'(V) - S(V)) = C(D^{**}(V) - D^*(V)) = w(V). \quad (8)$$

That is, $w(V)$ is the vertical distance between the pre- and post-change curves. Thus, if transactions costs are zero and $V = V_0$, the value of creditors' claims should rise to $B'_0 = B_0 + w$, and the value of equity should fall to $S'_0 = S_0 - w$.

When transactions costs are present, the situation must be viewed somewhat differently. In this case, tightened loan restrictions

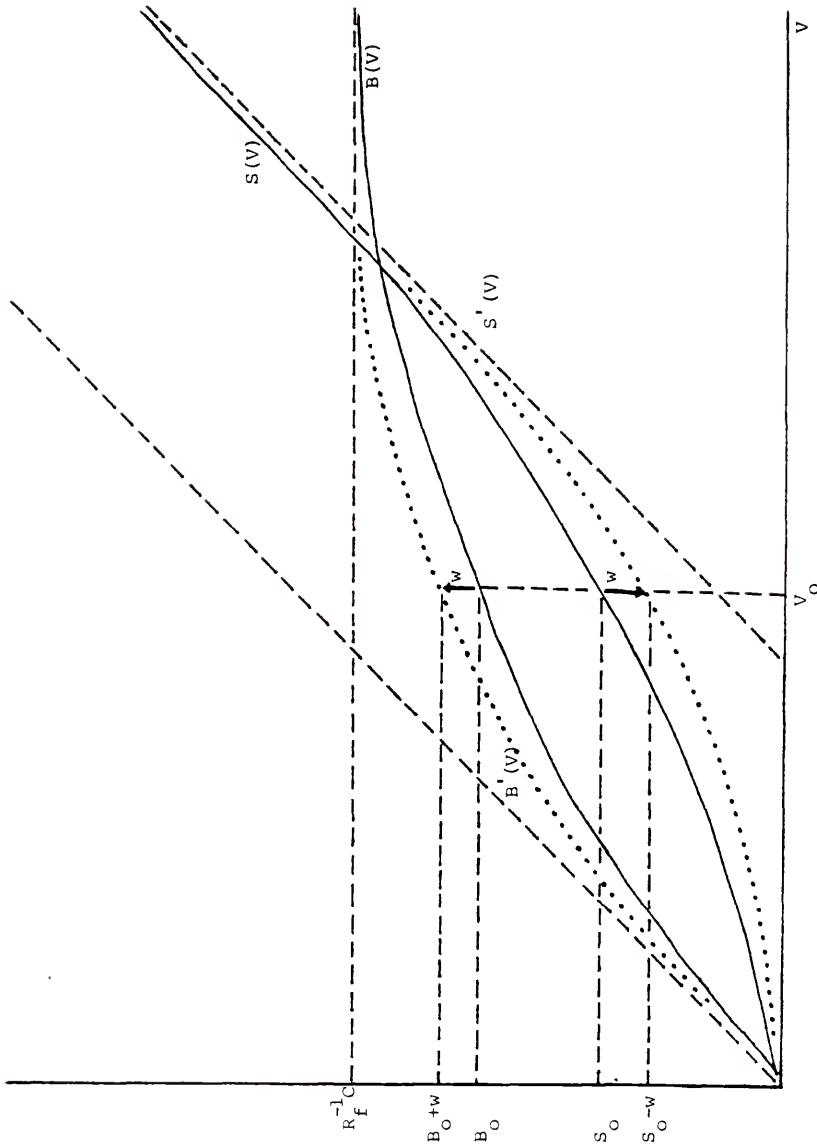


Figure 3-2
Pure Wealth Transfer Effect of a Mandated
Accounting Change Which Alters Loan Contract Terms

require that the post-change shareholder-creditor relationship be "located" along B' and S' , but the process of "getting there" may require an expenditure of the firm's resources. When creditors' claims are risky, changes in firm value are shared by creditors and shareholders. The relevant question is this: does the cross-sectional distribution of $\Delta S/S$ have the same properties in the presence of transactions costs as that hypothesized in the case where such costs are absent? In particular, should one still expect to observe the proposed "leverage by financial statement impact" interaction?

An answer to these questions can be approached by noting that shareholders, in order to "move" from B to B' , may adjust investment/financing programs using methods which are strictly unilateral (e.g., issue new equity, alter asset composition, decrease dividend rate) or, alternatively, using bilateral methods which require creditor cooperation (e.g., early redemption of loans, renegotiation of contract terms). Let the minimum transactions costs associated with unilateral and bilateral actions be represented as t_u and t_b , respectively. If shareholders choose to adjust θ using strictly unilateral methods, firm value will decline by $\Delta V = t_u$, equity value will decline by $\Delta S = (w + \alpha t_u)$, and the value of creditors' claims will increase by $\Delta B = (w - (1-\alpha)t_u)$. The proportion of t_u absorbed by shareholders, α , is related to the slope of the curve S . Thus, when S is very large (very small) relative to B , shareholders will bear virtually all (none) of these transactions costs. In any event, since the adjustments involved can be pursued without creditor cooperation, $(w + \alpha t_u)$ is the

maximum loss that can be imposed on shareholders as a result of the accounting change in question.

Since only the "zero transactions costs" assumption has been relaxed at this point, it can also be argued that $(w + \alpha t_u)$ is the minimum loss imposed on shareholders by a given adverse accounting change. Shareholders will be unwilling to adjust using bilateral methods unless the resulting equity value exceeds $S - w - \alpha t_u$. Creditors, on the other hand, will not participate in a bilateral solution unless the resulting loan value exceeds $B + w - (1 - \alpha)t_u$. For example, suppose that loans are to be redeemed at a price P with accompanying transactions costs t_b . Shareholders will require $V - P - t_b \geq S - w - \alpha t_u$. Creditors will require $P > B + w - (1 - \alpha)t_u$. These conditions can be written jointly as

$$0 < P - [B + w - (1 - \alpha)t_u] < t_u - t_b. \quad (9)$$

Basically, (9) says first that redemption is a possibility only if t_b is lower than the minimum transactions cost associated with unilateral shareholder action, second that the cooperation of creditors must be secured by a positive "premium", and third that this premium must be smaller than the achievable savings in transactions costs. Wealth maximizing creditors will simply set a premium which differs from $(t_u - t_b)$ by a small number. The resulting wealth effects will be (approximately) $S = -(w + \alpha t_u)$ and $B = w - (1 - \alpha)t_u + (t_u - t_b)$. A similar argument can be made in the case of loan renegotiation, so that in general the wealth effects of an adverse accounting change can be written (again approximately) as

$$\Delta S = -(w + \alpha t_u) \quad (10a)$$

$$\Delta B = w - (1 - \alpha)t_u + \max[0, t_u - t_b] \quad (10b)$$

Equations (10) indicate that ΔS will be approximately the same whether the response to the accounting change is "unilateral" or "bilateral", since wealth maximizing creditors will be able to absorb any cost savings achievable through bilateral action.

Thus, when the zero transactions costs assumption is relaxed, the appropriate characterization (in returns form) of the "contracting" share price impact of a mandated accounting change becomes

$$\frac{\Delta S}{S} = - \frac{\alpha t_u}{S} - \frac{C}{S} (D^{**} - D^*) \quad (11a)$$

$$= - \frac{\alpha}{S} t_u - \frac{1}{S} w. \quad (11b)$$

This means that the distribution of $\Delta S/S$ will have the same general shape in the presence of transactions costs as in their absence if the magnitude of $(\alpha t_u/S)$ is a constant or increasing function of the magnitude of (w/S) . This condition will obtain if one is willing to make the plausible assumption that the dollar magnitudes t_u and w are directly related. In this case, comparing the terms of (11b), t_u increases as w increases. It can also be shown that, while α itself approaches zero as S becomes very small, the ratio (α/S) becomes infinitely large.⁹ That is, (α/S) increases with $(1/S)$. Therefore, the product $(\alpha/S)t_u$ must increase as the product $(1/S)w$ increases.

In summary, it is argued that when an adverse accounting policy change is imposed, the cross-sectional distribution of $\Delta S/S$ should

have the same qualitative empirical properties whether transactions costs are present or absent.

The second assumption included in the analysis above is that all loan contracts, both within and across firms, contain equally restrictive accounting constraints. Actually, a given firm may have numerous loan agreements outstanding, which vary widely in the extent to which they place restrictions on shareholder activities subsequent to the contract date. And the general restrictiveness of contract terms may vary from one firm to the next. This suggests that a factor other than those already proposed (i.e., leverage and financial statement impact) must be taken into account in describing the distribution of share price effects following a mandated accounting change. In particular, it is argued here that if the financial statements of two firms are identically affected by an adverse accounting change, shareholders of that firm whose accounting-based loan covenants are most restrictive at the margin will experience the largest wealth effect as a result of the change.

Unfortunately, measurement of the restrictiveness of loan covenants at the margin is far from being a straightforward task. It is virtually impossible to learn from publicly available sources the precise terms of the possibly many loan contracts to which a given firm is party. Moreover, even if these data could be obtained, it is not clear that the various types of accounting constraints faced by an individual firm can be aggregated to provide a quantitative index of "restrictiveness" that would apply across firms. On the other hand, if one is willing to assume a direct relationship between the

restrictiveness of a firm's "average" loan contract and its "most restrictive" loan contract, a crude proxy for the latter can be developed. While public data concerning the terms of particular contracts is sparse, it is fairly easy to determine that proportion of C represented by loans subject to no accounting-based constraints whatsoever. Denote this proportion as λ , with $0 \leq \lambda \leq 1$. One can argue that when λ is large (small), the average and marginal restrictiveness of loan covenants is low (high), and the related share price effect should be small (large).

It can also be argued that when the third assumption -- that of "noncallability" -- is relaxed, the proportion λ is important from another perspective as well. Noncallability was initially assumed in order to guarantee that creditors would be able to take full advantage of the wealth transfer implied by an adverse accounting change. However, most real loans appear to include some provision for pre-mature redemption at the option of the borrower within a reasonably short period after the contract date.¹⁰ In the context of the current scenario, the "striking price" of such a call option would typically be the discounted value of C , at the rate of yield in force on the contract date, plus a penalty for early termination of the contract. Thus, the effect of relaxing the noncallability assumption is to increase the range of unilateral actions that shareholders may pursue in responding to the accounting change. If it is momentarily assumed that all outstanding loan contracts include accounting constraints (i.e., $\lambda=0$), that these loans can be called at an aggregate price P_C ,

and that the transaction itself entails costs t_c , then shareholders will prefer "calling" over other unilateral options whenever

$$(P_c - B) + t_c < w + \alpha t_u, \quad (12)$$

where other notation is as previously defined. If this condition is met, $(P_c - B + t_c)$ becomes the maximum loss that shareholders may sustain as the result of an adverse accounting change.

The magnitude of $(P_c - B)$ in (12) depends heavily on the spread between current interest rates and those which prevailed on the contract date. If current rates are relatively low, $(P_c - B)$ should be close to zero. If current rates are relatively high, $(P_c - B)$ may become quite large. Since interest rates were historically high during much of the period to be examined in this study, it can be argued that, at least under the conditions just stated, exercising the option to call existing loans would tend to be a very costly response from the shareholders' point of view, and therefore that relaxing the noncallability assumption should have little real effect on the analysis presented thus far. On the other hand, if λ is large -- that is, if most of a firm's loans are not subject to accounting restrictions -- then calling those loans which are subject to accounting restrictions may be the minimum cost shareholder response to an adverse accounting change even during a period in which loans are "trading" at a substantial discount.

The final assumption which requires further comment -- and perhaps the most important of the four -- is that maximizing the value of a given loan is, from the creditor's point of view, consistent with

wealth maximization. One might think of an adverse accounting change as creating a wealth transfer potential which creditors may or may not choose to realize. Wealth maximizing creditors would always choose to realize this potential if the act of so choosing had no external effects on other (present or future) contracts. Where external effects are present, it is conceivable that the gain to be achieved by realizing such a wealth transfer could be partially or completely offset by losses connected with other contracts. In the latter case, a rational creditor would be willing to renegotiate the terms of a loan contract in order to neutralize the effect of the accounting change, and the costs imposed on shareholders could, in the final analysis, be no greater than the direct costs associated with the renegotiation process. In short, the pattern of share price behavior explicated in the preceding pages can be viewed as one which is possible, but which will be guaranteed to materialize only if external effects associated with the realization of "shareholder to creditor" wealth transfers are generally negligible in magnitude.

One distinction that may be useful in this context is that between privately placed loans (which for present purposes can be defined to include unregistered securities held by insurance companies and other institutional lenders, and term loans provided by commercial banks) and public debt issues (those registered under the Trust Indenture Act of 1939). Loans in the latter category are more likely to be widely held by creditors whose relationship with the firm is essentially anonymous. In this situation, any external effects on other contracts to which these creditors are (or will be) a party

should be minimal. Thus, "public" creditors should be willing to fully exploit the wealth transfer potential inherent in an adverse accounting change.

There is, on the other hand, a distinct possibility that private creditors may not be so willing. For example, Zinbarg (1975), apparently speaking in his capacity as an executive of the Prudential Insurance Co. of America's Bond and Commercial Loan Department, has this to say regarding the modification of contract terms:

. . . some [private placement] lenders will make modifications only in exchange for a higher interest rate or a tightening of other parts of the agreement. However, private placement lenders generally view the Loan Agreement as a living document destined to be modified periodically to take account of changing circumstances. Accordingly, these lenders make most modifications routinely, with no quid pro quo exacted from the borrower unless the proposed corporate action will compromise the lender's margin of safety. (Zinbarg, 1975, p. 35)

The reason for this Marquess of Queensbury-like behavior is also clarified in Zinbarg's article:

. . . the bulk of each year's new loans are additional credits extended to existing borrowers. Indeed, the very fact that continuous communication is maintained through the loan modification process makes both parties more comfortable with each other, and facilitates the repeated financing of borrower's long term growth. (p. 52)

These remarks suggest, in fact, that the direct placement creditor/borrower relationship is an enduring one which is costly to replicate, and therefore that the creditor's incentive to capitalize on the

wealth transfer potential of an adverse accounting change may be virtually nil.

Hayes (1977), in a lengthy summary of the term lending policies of eight large mid-western banks, indicates that bankers take a similar view of the creditor/borrower relationship:

Because loan officers as a rule maintain close contacts with their customers, often acting almost as regular financial advisors, they generally seek every conceivable means of bringing a borrower through periods of difficulty without permanent damage to his earning capacities. Thus flexibility in adjusting terms of loan agreements to changed conditions is regarded as an axiom of policy. (Hayes, 1977, p. 124)

On the other hand, the banker's view of the creditor/borrower relationship in the case of other institutional creditors is curiously at odds with that expressed by Zinbarg:

. . . life insurance companies and pension funds use basically the same approach to long-term direct loans as to purchases of bonds in the market. There is seldom any expectation of a continuing relationship or of ancillary business. As the relation by the nature of the case is impersonal, these institutional investors have an understandably strong preference for avoiding frequent negotiations for changes in the terms of loan agreements. (p. 124)

Each of these sets of remarks may contain a certain element of "window dressing." However, they at least suggest that, other things equal, one may expect to observe a larger (smaller) share price effect in response to an adverse accounting change when the greatest part of a firm's debt is publicly (privately) placed. In passing, it should be mentioned that this same distinction was proposed by Leftwich, but

for a different reason. Leftwich argued that the transactions costs involved in renegotiating and amending a public indenture would greatly exceed those necessary to amend a private loan agreement.¹¹

It has been suggested here that the incentives inherent in the "public" and "private" situations may be substantially different. The effects of these arguments are, however, complementary.

3.5 Hypotheses

A number of arguments have been made in preceding sections of this chapter. First, because the investment/financing opportunity set available to shareholders is frequently restricted by the terms of existing loan contracts, and because these restrictions often take the form of constraints on financial statement numbers and relationships, it was suggested that the effect on equity values of the "lessee" accounting policy events of the 1973-1976 period -- culminating in more stringent capitalization conditions and the necessity for retro-active application -- was potentially adverse. Second, it was proposed that, if one is willing to make certain simplifying assumptions about the characteristics of loans and the attitudes of creditors, a reasonably uncomplicated model of the cross-sectional distribution of such "share price effects" would result. In this model, the magnitude of the adverse share price effect for a given firm depends jointly on the extent to which the firm is leveraged and on the impact of the policy change on financial statement numbers and relationships. Third, it was acknowledged that the assumptions underlying this model could well be excessively unrealistic, and two

further variables were identified which might be expected to have a moderating influence on the distribution of share price effects.

These arguments can be distilled into a number of testable hypotheses. First, suppose that a group of firms has been partitioned according to "degree of leverage" and "degree of financial statement impact," as indicated in Table 3-1. Define u_{jt} as the non-systematic component of the realized return on firm j 's equity in period t , and let

$$\mu_k = E(u_{jt}) | j \in \text{cell } k.$$

Table 3-1
Partitioning Scheme

		Degree of Leverage	
		Low	High
Degree of Financial Statement Impact	Large	μ_1 (Cell 1)	μ_2 (Cell 2)
	Small	μ_3 (Cell 3)	μ_4 (Cell 4)

If t can be identified as the period in which the effect of an adverse lessee accounting change is impounded in prices, then equation (7) suggests the following hypotheses:

Null Hypothesis

$$H_0: \mu_1 = \mu_2 = \mu_3 = \mu_4$$

Alternative Hypotheses

$$H_{a1} : \mu_2 - \mu_1 < 0$$

$$H_{a2} : \mu_3 - \mu_1 < 0$$

$$\begin{aligned} H_{a3} : (\mu_4 - \mu_3) - (\mu_2 - \mu_1) \\ = (\mu_4 - \mu_2) - (\mu_3 - \mu_1) < 0 \end{aligned}$$

Beyond the simple cell mean differences indicated by equation (7), the analysis of this chapter also suggests that the magnitude of u_{jt} should:

H_{a4} : vary directly with λ' , the proportion of the firm's debt which is subject to accounting-based restrictions; and

H_{a5} : vary directly with P , the "publicly traded" proportion of such debt.

3.6 Notes

1. These measures were deflated using (apparently) the pre-study period market value of equity.
2. "Firm size" plays no direct role in the Leftwich contracting cost argument. This variable was included in the various cross-sectional regressions in order to control for (a) an empirical relationship between size and risk adjusted returns, as observed by Banz (1980), and (b) the "political costs" hypothesized by Watts and Zimmerman (1978).
3. The data gathered by Abdel-khalik, Ajinkya, and McKeown (1981) and by Pfeiffer (1980) provide ample support for this contention.
4. The "agency costs" associated with debt financing are those costs which arise precisely because of this conflict of interests. If the shareholder/creditor conflict of interests could be completely controlled via an appropriately constructed contract, the related agency costs would simply be the sum of the monitoring and bonding costs required to administer the contract. If only partial control is feasible, agency costs include a third component, referred to by Jensen and Meckling as the "residual loss," equal in value to the amount of wealth which shareholders may legally divert from creditors, given the terms of the contract.
5. For a general discussion of loan covenants, see Smith and Warner (1979). For a very complete discussion, see Commentaries on Indentures (1971), a detailed study undertaken by the American Bar Foundation.
6. Leftwich examined ten sample loan agreements provided by five large insurance companies. In the present study, fifteen recent public debenture indentures ("public" in the sense that the associated debentures were registered under the Trust Indenture Act of 1939) were examined, over half of which included negative covenants based on accounting numbers.
7. No exceptions to this observation were reported by Leftwich, and no exceptions were found in the public indentures reviewed here. While a joint AICPA/American Bankers Association committee (AEA, 1968) recommended a "frozen GAAP" approach to loan covenants, conversations with practicing public accountants indicate that such an approach is not widely followed, if indeed it is followed at all. Fogelson (1978) cites one instance of a public indenture in which GAAP is frozen (Macmillan 8.85% sinking fund debentures, due 2001). It is interesting to note that this indenture, which is dated a few weeks prior to the adoption of SFAS 13, opted for "frozen GAAP" primarily because of pending changes in lessee accounting.

8. The loan agreement thus far described obviously differs from the typical "real-world" contract in other ways as well. Few real loans, excepting those of very short maturity, are of the pure discount variety, and failure to make schedule intermediate payments -- e.g., those which meet interest and sinking fund requirements -- constitutes a serious event of default. One would suspect that a contract calling for systematic loan amortization helps to reduce, but does not eliminate, agency problems associated with the loan.
9. As pointed out previously, the magnitude of α is related to the slope of the curve S in Figures 3-1 and 3-2. The curve S can be viewed as a segment of an hyperbola centered on $V = 0$, and it can be shown that as V (and S) approach zero, the ratio (S_V/V) goes to infinity.
10. Virtually all loans registered under the Trust Indenture Act of 1939 (i.e., "publicly traded" loans) are callable. Term loans from commercial banks are commonly redeemable at the option of the debtor, providing that a premium or penalty is paid (Simmons, 1972). The "least callable" of typical loan contracts would appear to be notes which are directly placed with insurance companies and certain other institutional lenders. These agreements permit "optional prepayments," but only to a limited extent. Total prepayment is normally allowed if the lender refuses the borrower's request for additional funds and if the borrower has in hand an offer from another lender for an amount equal to the loan balance plus the additional amount requested (Zinbarg, 1975). The latter option is sometimes expanded to include situations in which the lender refuses to consent to a transaction which is prohibited by one or more negative covenants (including accounting constraints) included in the agreement (Fryling, 1965).
11. Zinbarg (1975) suggests that in many cases, no more than a telephone call and an exchange of letters is required to obtain a waiver of a negative covenant in the direct placement loan situation. The costs required to achieve a formal amendment of the agreement are also, presumably, small. On the other hand, amendment of a "public" loan agreement typically requires the consent of the holders of two thirds of the outstanding bonds -- an expensive undertaking if the bond are widely held.

CHAPTER 4

SAMPLE SELECTION AND PRELIMINARY TESTS OF EVENT-RELATED ABNORMAL PERFORMANCE

4.1 Introduction

The discussion of Chapters 2 and 3 raises questions which can be investigated empirically on two distinct levels. First, did the various lease-related accounting standards of the 1973-1976 period have an adverse impact on lessee share prices? And second, if an adverse impact did indeed result, is the cross-sectional distribution of that impact consistent with that which would be expected under the "contracting hypothesis" as it applies to loan contracts? These questions will be explored using a two-stage research design. In the general parlance of capital market studies, the first stage will be devoted to an assessment of the "event-related abnormal performance" of lessee firms. That is, the share price behavior for a sample of lessee firms, relative to that for a sample of non-lessee firms, will be examined for short periods surrounding certain critical events which occurred during the standard setting process. The purpose of this examination is to determine whether the relative price behavior of lessee shares differed in any significant way from that observed in "non-event" periods of similar duration. The second stage of the

research design, consisting of a more detailed cross-sectional analysis, will be described in Chapter 5.

The present chapter reports the procedures used in, and the results of, preliminary tests of event-related abnormal performance. Matters of sample selection are discussed in Section 4.2. Section 4.3 describes the data employed in these tests. Section 4.4 is devoted to a discussion of research method. Empirical results are reported and analyzed in Section 4.5. Additional discussion of these results, and a comparison with the results of previous studies, is provided in Section 4.6.

4.2 Sample Selection

The purpose of the sample selection procedure followed in this study was to form approximately equal-sized samples of lessee and non-lessee firms suitable for use both in preliminary event-related abnormal performance tests and in the cross-sectional tests to be described in Chapter 5. The basic problem to be solved in selecting such samples is to determine how "lessee" firms are to be distinguished from "non-lessee" firms. The classification criterion adopted for this study is based on the ratio (NCL/TA) , where NCL is the present value of noncapitalized financing leases disclosed (per ASR 147) for the first fiscal year end following 1973, and TA is the book value of total assets on the same date. A firm was classified as a lessee (non-lessee) if the value of this ratio was greater than 0.05 (less than 0.01). While somewhat arbitrary, this criterion seems

intuitively reasonable. Moreover, it bears a family resemblance to those adopted in most previous studies.

With this criterion in hand, the Standard and Poors Compustat file was searched for potential lessee and non-lessee sample firms. In the process, a firm was eliminated from further consideration if it was

- i. a financial institution or a public utility;
- ii. lacking basic Compustat data for any year during the period 1973-1978; or
- iii. not listed on NYSE or AMEX during the same period.

Financial institutions were excluded from consideration because such firms tend to be lessors, and thus were potentially affected by aspects of lease accounting standards not contemplated in the present study. Public utilities were excluded in order to insure that the final sample included primarily unregulated firms. The remaining criteria for exclusion simply establish minimum data requirements.

The Compustat search produced a potential lessee sample of 320 firms and a potential non-lessee sample of 737 firms. In order to insure that the final samples would consist of firms having sufficiently lengthy time series of returns for all subsequent analysis, each potential sample was then checked against the CRSP Daily Stock Returns File to eliminate firms whose first (last) recorded daily return occurred after December 31, 1968 (before December 31, 1978). This procedure reduced the potential lessee and non-lessee samples to 235 and 555 firms, respectively.

At this point the potential non-lessee sample was reduced, through a random selection process, to 235 firms. For each remaining firm in each potential sample, weekly returns were then calculated over the (inclusive) calendar interval 1969-1978, and, in order to insure that weekly time series would be sufficiently "dense" for all subsequent analysis, a last filter was applied. Firms were eliminated from each potential sample in cases where the proportion of missing weekly returns over the 1969-1978 interval exceeded five percent.

The selection process just described, which is summarized in Table 4-1, resulted in a final lessee sample of 197 firms and a final non-lessee sample of 195 firms. A summary of the sample distributions of total accounting assets and the ratio (NCL/TA) -- as of the first fiscal year-end following November 30, 1973 -- is provided in Table 4-2.

4.3 Data

The data to be employed in the preliminary tests reported in the present chapter consist of (a) the samples of lessee and non-lessee firms whose selection was just described, (b) a time series of weekly stock returns for each firm in each sample, and (c) a list of "critical events" which occurred during the 1973-1976 lessee accounting standard-setting process. Matters relating to the calculation of weekly returns and to the identification of critical events are discussed in this section.

Table 4-1
Summary of Sample Selection Procedure

	<u>Lessee</u>	<u>Non-Lessee</u>
<u>Compustat</u> firms not eliminated in accordance with criteria i-iii	320	737
Firms whose first or last recorded CRSP daily return fall within the 1969-1978 calendar interval	(85)	(182)
Random selection of non-lessee firms	-	(320)
More than 5% of weekly returns missing during the 1969-1978 interval	<u>(38)</u>	<u>(40)</u>
Final sample size	<u>197</u>	<u>195</u>

Table 4-2
Distribution of TA and (NCL/TA)

Decile	Total Assets (TA) ^a		PVNCL/TA
	L	NL	L
0 (min)	4.60	2.80	0.05
1	24.84	13.73	0.06
2	40.54	29.52	0.08
3	65.63	50.21	0.09
4	92.30	100.42	0.11
5	155.03	158.54	0.15
6	237.86	236.29	0.18
7	361.00	378.52	0.28
8	659.25	724.26	0.40
9	1,432.62	1,572.20	0.57
10 (max)	9,082.25	20,296.90	1.20
Avg.	520.90	651.95	0.24
Std. Deviation	1,057.46	1,893.63	0.23

^a \$Millions

Returns

The tests to be described in succeeding sections of this chapter and in Chapter 5 are based on an analysis of weekly stock returns, and a question that should be asked and answered initially is: why weekly, rather than daily or monthly returns? Monthly returns are dispensed with in the present research because some of the critical events to be studied are spaced quite closely in time, and also because a contemplated correction for the "nonstationarity problem" discussed in Chapter 2 requires the availability of frequent return observations over relatively small time intervals. While both of these objections to monthly returns could be avoided by focusing on daily returns, the latter are subject to a potentially large degree of measurement error due to the "nonsynchronous trading problem" elucidated in Scholes and Williams (1977). Weekly returns are felt to represent a reasonable compromise between these extremes, in that the frequency of measurement should be sufficient for present purposes, while the error in measurement should be considerably reduced relative to that inherent in daily returns.

For each lessee and non-lessee sample firm, a time series of weekly returns was constructed for the calendar period 1969-1978, based on daily returns obtained from the CRSP Daily Stock Return File. Generally, the return for firm j during week t was calculated as

$$R_{jt} = \ln \prod_{d=1}^L (1 + r_{jd}) \quad (1)$$

where $d = 1$ and $d = L$ represent the first and last trading days of week t (normally Monday and Friday), and r_{jd} is the daily return

recorded for the d^{th} day. One exception was made in applying this calculation rule: if firm j 's daily return for the last trading day of week t was not available on CRSP, then j 's weekly returns for weeks t and $t + 1$ were classified as "missing data."

Critical Events

As indicated in Chapter 2, SFAS 13 can be seen as the culmination of a process which was both lengthy and complex. This process can be envisioned as a series of events, each of which signals changes in the likelihoods associated with various possible final outcomes. If it is supposed first that at least one such outcome had implications for lessee share prices which were other than neutral, and second that information is impounded in prices with reasonable efficiency, then one would expect to observe an average adjustment to lessee share prices in connection with at least some events in the series. And if every relevant event in the series could be identified, one could then make an overall assessment of the average share price impact of the process taken as a whole.

The "sample" of events to be examined in the present study, which is limited to the 1973-1976 calendar period, was identified by reference to the issuance date of actual accounting standards, to "official" histories of the standard setting process included in the FASB discussion memorandum and in the first appendix to SFAS 13, and finally to the press coverage accorded to lessee accounting developments by the Wall Street Journal. The end result of this "search for events" was the list of sixteen events summarized in Table 4-3. As a

Table 4-3

Index of
Events Connected with the Evolution
of Lessee Accounting Standards
-- Calendar 1973-1976 --

Event	Week of	Description
1	4/2/73	<u>Monday</u>. FASB announces initial agenda, which includes a comprehensive examination of lease accounting methods and disclosures. <u>Thursday</u>. FASB announcement covered by WSJ.
2	5/18/73	<u>Thursday</u>. SEC official announces that an SEC pronouncement requiring <u>pro forma</u> capitalization disclosures is likely to be forthcoming. <u>Friday</u>. Announcement covered by WSJ.
3	6/4/73	<u>Wednesday</u>. SEC issues SAR 5401, proposing increased disclosure by lessees, including a requirement for <u>pro forma</u> capitalization. Accounting profession is criticized for failing to act quickly enough to adopt new standards in this area. <u>Thursday</u>. Covered by WSJ.
4	6/11/73	<u>Thursday</u>. APB 31 adopted by Accounting Principles Board. Covered same day by WSJ.
5	10/1/73	<u>Friday</u>. SEC issues <u>ASR 147</u>.
6	10/8/73	<u>Monday</u>. Issuance of <u>ASR 147</u> covered by WSJ.
7	7/1/74	<u>Tuesday</u>. FASB issues lease accounting discussion memorandum and calls for public hearings.

Table 4-3 (continued)

Event	Week of	Description
8	7/8/74	<u>Wednesday</u> . Covered by WSJ.
9	11/18/74	<u>Monday-Wednesday</u> . FASB holds public hearings.
10	11/25/74	<u>Monday</u> . WSJ coverage of public hearings.
11	8/25/75	<u>Tuesday</u> . FASB issues first exposure draft. <u>Friday</u> . Covered by WSJ.
12	11/24/75	<u>Tuesday</u> . FASB postpones issuance of final standard until early in 1976. <u>Wednesday</u> . Covered by WSJ.
13	6/1/76	<u>Tuesday</u> . FASB announces that a new exposure draft, including major changes in proposed lease accounting rules, is to be issued around mid-year. <u>Wednesday</u> . Announcement covered by WSJ.
14	7/19/76	<u>Thursday</u> . FASB issues second exposure draft.
15	7/26/76	<u>Wednesday</u> . Second exposure draft covered by WSJ.
16	11/29/76	<u>(?)</u> . FASB issues <u>SFAS 13</u> . <u>Friday</u> . Covered by WSJ.

matter of clarification, it should be noted that, because the statistical analysis to follow will be based on weekly stock returns, multiple actual events occurring during the same trading week are categorized in Table 4-3 as a single event. This procedure should not be a source of confusion: no more than two actual events occur in any given trading week, and in all such cases the second event is simply press coverage of the first.

While it is difficult to know whether Table 4-3 encompasses all "relevant events" which occurred during the 1973-1976 standard-setting period, it does include all major lessee accounting policy initiatives undertaken during the period by the APB, SEC, and FASB. And, taking the accuracy of the Wall Street Journal Index as given, it also includes all major coverage of lessee accounting issues by what is clearly the most influential and widely read component of the nation's financial press. Accordingly, the analyses to be described in the remainder of this study are predicated on the assumption that the list of events presented in Table 4-3 is reasonably comprehensive.

4.4 Research Design for First-Stage Tests

In this section, some basic assumptions are made concerning the cross-sectional pattern of security returns. A basic test procedure is also described, the purpose of which is to detect the presence, and to estimate the magnitude of, any "abnormal returns" to lessee firms during brief periods surrounding the lessee accounting events identified in section 4.3

A Cross-Sectional Model of Security Returns

In order to test hypotheses concerning the relative performance of lessee share prices during specific time periods associated with the events described in the preceding section, a cross-sectional model of security returns must first be specified. The cross-sectional model adopted for current purposes is based on the assumption that ex post returns are "generated" by a one factor process of the form

$$\tilde{R}_{jt} = E_{jt} + b_{jt}\tilde{\phi}_t + \tilde{u}_{jt}, \quad (2)$$

with

$$E(\tilde{u}_{jt}) = 0; \quad (2a)$$

$$E(\tilde{u}_{jt}\tilde{u}_{kt}) = \begin{cases} \sigma_j^2 & j = k \\ 0 & j \neq k \end{cases}; \quad (2b)$$

$$E(\tilde{u}_{jt}b_{jt}) = 0; \quad (2c)$$

$$E(\tilde{\phi}_t) = 0; \quad (2d)$$

$$E(\tilde{\phi}_t^2) = 1; \quad (2e)$$

$$\text{and } E(\tilde{u}_{jt}\tilde{\phi}_t) = 0. \quad (2f)$$

In equation (2), $\tilde{R}_{jt}$ is the return on security j during period t , E_{jt} is the ex ante expectation of $\tilde{R}_{jt}$, $\tilde{\phi}_t$ is the unanticipated component of an unspecified economic "factor," b_{jt} measures the sensitivity R_{jt} to $\tilde{\phi}_t$, and $\tilde{u}_{jt}$ is an independently distributed disturbance.

Ross (1977) shown that, given a reasonably weak set of additional assumptions¹, (2) - (2f) imply a linear relationship between E_{jt} and

b_{jt} :

$$E_{jt} = \alpha_{1t} + \alpha_{2t} b_{jt}. \quad (3)$$

Thus, (2) can be rewritten as

$$\tilde{R}_{jt} = \alpha_{1t} + (\alpha_{2t} + \tilde{\phi}_t) b_{jt} + \tilde{u}_{jt}. \quad (4)$$

Equation (4) is the basic cross-sectional representation of returns that will be employed in the present study. However, (4) is not suitable for direct use in an empirical context. Since ϕ_t has not been precisely defined, $b_{jt} = \text{cov}(\tilde{R}_{jt}, \tilde{\phi}_t)$ is not directly observable. A quantity which can (at least potentially) be observed is the "beta coefficient" common to most security price impact studies:

$$\beta_{jt} = \frac{\text{cov}(\tilde{R}_{jt}, \tilde{R}_{mt})}{\text{var}(\tilde{R}_{mt})} \quad (5)$$

where R_{mt} is the return during period t on a highly diversified portfolio of common stocks. Using (2) - (2f) and the definitions of variance and covariance, $\beta_{jt} = (b_{jt}/b_{mt})$, so that (4) can be alternately expressed as

$$\tilde{R}_{jt} = \alpha_{1t} + [b_{mt}(\alpha_{2t} + \tilde{\phi}_t)]\beta_{jt} + \tilde{u}_{jt}, \quad (6)$$

or, more simply, as

$$\tilde{R}_{jt} = a_{1t} + a_{2t}\beta_{jt} + \tilde{u}_{jt}. \quad (7)$$

In passing, it should be pointed out that (7), while formally equivalent to the cross-sectional regression used by Fama and MacBeth (1973) and others to test the Sharpe-Lintner-Black capital asset pricing model, requires no assumptions concerning the mean-variance efficiency

of the portfolio m . In the context of the lessee accounting share price studies reviewed in Chapter 2, (7) is also identical to cross-sectional model used by May, Harkins, and Rice (1978).

Assessing the Relative Performance of Lessee and Non-Lessee Shares

The question to be explored in the remainder of this chapter is: after controlling for the sensitivity of returns to ϕ , did the returns to lessee shares during periods surrounding events 1-16 differ significantly (on average) from the returns to non-lessee shares? In terms of (7), answering this question corresponds to testing the null and alternative hypotheses.

$$H_0: \bar{u}_{L,t} = \bar{u}_{NL,t}$$

$$H_a: \bar{u}_{L,t} \neq \bar{u}_{NL,t}$$

where L and NL are categories representing lessee and non-lessee firms, respectively. One way to conduct such a test is to estimate the parameters of the cross-sectional regression

$$R_{jt} = a_{1t} + a_{2t}\beta_{jt} + a_{3t}Z_j + w_{jt}, \quad (8)$$

where

$$Z_j = \begin{cases} 1 & \text{if } j \in L \\ 0 & \text{if } j \in NL \end{cases}$$

The coefficient a_{3t} in (8) is equivalent to $(\bar{u}_{L,t} - \bar{u}_{NL,t})$, and an appropriate method of testing H_a is to determine whether an unbiased estimator of a_{3t} differs significantly from zero at time t .

Test Procedure

In order to clarify the discussion of statistical procedures which will follow, a "temporal" framework needs to be established. Accordingly, the study period to be examined is defined as the 236 week period beginning 22 weeks prior to Event 1 and ending 22 weeks after Event 16. In order to simplify further references to the position in time of a given event, the weeks of the study period are arbitrarily numbered 200-435. In calendar terms, the study period extends from October 30, 1972 to May 6, 1977.

Within the study period, event weeks are the trading weeks associated with Events 1-16, as enumerated in Table 4-3. Event periods are identical to event weeks except in those cases in which two event weeks are adjacent calendar weeks. Where the latter condition occurs, the related event period is defined to include both weeks. Event weeks and event periods are summarized in Table 4-4. Finally, any week during the study period which (a) is not an event week, and (b) does not fall in three week intervals preceding or following an event period, is defined to be a non-event week.

With this simplified calendar in hand, a statistical analysis of event-related abnormal performance can now be undertaken. In the first phase of this analysis, it will be assumed that β_j is stationary for all j , and ordinary least squares techniques will be used to estimate the parameters of the regression

$$R_{jt} = a_{1t} + a_{2t}\beta_{jt} + a_{3t}Z_j + w_{jt} \quad (9)$$

Table 4-4
Event Weeks and Event Periods

Event	Week #	Event Period	Brief Description
1	222	1	FASB agenda announcement
2	228	2	SEC announcement
3	231	3	<u>APB 31</u>
4	232		
5	248	4	<u>ASR 147</u>
6	249		
7	287	5	FASB discussion memorandum
8	288		
9	307	6	FASB public hearings
10	308		
11	347	7	First exposure draft
12	360	8	Postponement announcement
13	387	9	Modification announcement
14	394	10	Second exposure draft
15	395		
16	413	11	<u>SFAS 13</u>

- for (a) each week of the 236 week study period; and
 (b) for seven or eight week intervals corresponding to each of the eleven event periods defined in Table 4-4.

In regressions of type (a), R_{jt} is simply the return to security j during week t ; in those of type (b), R_{jt} will be measured as the average weekly return to security j over the three weeks preceding the event period, the event period itself, and the three weeks following the event period. An estimate of β_j in (8) and (9), $\hat{\beta}_j$, will be calculated for each security via an OLS "market model" regression

$$R_{jt} = \alpha_j + \beta_j R_{mt} + \epsilon_{jt}, \quad (10)$$

based on data for the 100 weeks immediately preceding the study period. For this purpose, R_{mt} is defined as the weekly return on the CRSP equally weighted index of NYSE and AMEX stocks.

The estimated coefficients $\hat{a}_{3t}$ from regressions (9) will be examined in order to make some tentative observations concerning the relative behavior of lessee and non-lessee equity returns during periods associated with lessee accounting events. It should be noted, however, that econometric problems abound when (9) is estimated in the manner just described, and that the effect of those problems may be to bias either $\hat{a}_{3t}$ or its estimated standard error. In a second phase of the analysis, therefore, these problems will be explicated, and, insofar as data and theory permit, corrected.

4.5 Empirical Results

In this section, preliminary estimates of the coefficients of regressions (9), obtained by following the procedure just outlined, are reported and briefly discussed. A number of econometric problems associated with that procedure are then outlined, and a revised procedure, designed to correct as many of these problems as possible, is described. A second (and hopefully more accurate) round of estimates, obtained by following the revised procedure, is then presented.

Preliminary Results

A distributional summary of some results from "market model" regressions (10) is provided in Table 4-5. The table shows quartiles, sample means, and standard deviations of estimates $\hat{\beta}_j$, estimated standard errors $\hat{s}_{\beta_j}$, and residual variances s_e^2 . While the sample mean $\hat{\beta}_j$ for the total sample of 392 firms is approximately 1.0, there appears on average to be a marked difference between the $\hat{\beta}_j$ of lessee and non-lessee subsamples, with the former being higher. (The t statistic for $\bar{\hat{\beta}}_L - \bar{\hat{\beta}}_{NL}$ is 2.54). Other results from these regressions, not shown in the table, are quite in line with those of many other large sample studies. Durbin-Watson statistics center on 2.0, and are significant only at the extreme tails of the empirical distribution. Values of R^2 range from 0.002 to 0.402, with a median of 0.165.

Using the $\hat{\beta}_j$ thus obtained as proxies for the related true values, cross-sectional regression (9) was estimated for each week of

Table 4-5

Distribution of "Market Model" Regression Estimates
Based on 100 Weekly Return Observations
Prior to Study Period

[illegible]

the study period and for each of the event period intervals defined in the preceding section. The results of these regressions are presented in the following way. Estimated coefficients for event weeks 1-16 are given in Table 4-6. Estimated coefficients for event periods 1-11 are given in Table 4-7. Finally, a summary of the estimated coefficients for all non-event weeks is given in Table 4-8.

In discussing these results, a t statistic with absolute value 1.65, corresponding to the 10% confidence level for a two-tailed test, is adopted as the criterion for "statistical significance." Looking first at Table 4-6, it appears that lessee equities earned significant abnormal returns, ranging in magnitude from 0.90% to 2.40%, during five of sixteen "event weeks." In four of these weeks, the abnormal performance is negative:

- (1) EW1, the week in which the FASB announced its initial agenda;
- (2) EW4, the week in which APB 31 was adopted;
- (3) EW6, the week in which the adoption of ASR 147 was reported in the Wall Street Journal; and
- (4) EW9, the week of the FASB public hearings.

Abnormal performance was positive during EW12, the week in which the FASB, in response to controversy surrounding its first exposure draft, postponed the adoption of a final lessee accounting standard.

To allow for the possibility that the market either anticipated, or reacted sluggishly to, some or all of the individual events considered in Table 4-6, Table 4-7 provides regression results for eleven "event period intervals," each consisting of an event period

Table 4-6

Estimated Coefficients of Cross-Sectional Regression

$$R_{jt} = a_{1t} + a_{2t}\beta_j + a_{3t}Z_j + w_{jt}$$

Event Weeks 1-8

	Event Week			
	1	2	3	4
$\hat{a}_{1t}$	-0.011	-0.020	0.009	-0.018
$t(\hat{a}_{1t})$	-1.66	-2.41	1.13	-2.21
$\hat{a}_{2t}$	-0.011	-0.049	0.004	0.017
$t(\hat{a}_{2t})$	-1.82	-6.75	0.54	2.38
$\hat{a}_{3t}$	-0.009	0.001	-0.005	-0.011
$t(\hat{a}_{3t})$	-1.81	0.093	-0.76	-1.83
R^2	0.02	0.11	0.00	0.02
F	3.78	23.15	0.39	4.04
	Event Week			
	5	6	7	8
$\hat{a}_{1t}$	0.017	0.021	-0.032	-0.012
$t(\hat{a}_{1t})$	1.77	2.56	-4.51	-1.34
$\hat{a}_{2t}$	0.028	0.002	-0.004	-0.003
$t(\hat{a}_{2t})$	3.34	0.27	-0.66	-0.44
a_{3t}	-0.006	-0.024	-0.001	0.006
$t(a_{3t})$	-0.84	-3.82	-0.18	0.98
R^2	0.03	0.04	0.00	0.00
F	5.69	7.34	0.26	0.53

Table 4-6 (continued)

Event Weeks 9-16

	Event Week			
	9	10	11	12
$\hat{a}_{1t}$	-0.035	0.012	0.030	0.010
$t(\hat{a}_{1t})$	-4.71	1.38	4.42	1.50
$\hat{a}_{2t}$	-0.005	-0.013	0.001	-0.001
$t(\hat{a}_{2t})$	-0.75	-1.62	0.09	-0.12
$\hat{a}_{3t}$	-0.010	-0.004	0.007	0.010
$t(\hat{a}_{3t})$	-1.83	-0.65	1.31	1.88
R^2	0.01	0.01	0.00	0.01
F	2.19	1.71	0.89	1.77
	Event Week			
	13	14	15	16
$\hat{a}_{1t}$	-0.001	-0.000	-0.000	-0.005
$t(\hat{a}_{1t})$	-0.18	-0.00	-0.07	-0.85
$\hat{a}_{2t}$	-0.011	-0.014	-0.014	0.004
$t(\hat{a}_{2t})$	-2.48	-2.02	-2.90	0.74
$\hat{a}_{3t}$	0.004	0.008	-0.001	0.006
$t(\hat{a}_{3t})$	1.20	1.40	-0.15	1.12
R^2	0.02	0.01	0.02	0.01
F	3.47	2.70	4.37	1.04

Table 4-7

Estimated Coefficients of Cross-Sectional Regression

$$R_{jt} = a_{1t} + a_{2t}\beta_j + a_{3t}Z_j + w_{jt}$$

Event Periods 1-8

	Event Period Interval			
	1	2	3	4
$\hat{a}_{1t}$	-0.005	-0.003	-0.004	0.009
$t(\hat{a}_{1t})$	-2.41	-1.39	-2.23	3.99
$\hat{a}_{2t}$	-0.006	-0.014	-0.010	0.001
$t(\hat{a}_{2t})$	-2.94	-6.76	-5.88	0.34
$\hat{a}_{3t}$	-0.006	-0.003	-0.006	-0.006
$t(\hat{a}_{3t})$	-3.67	-1.90	-4.80	-3.25
R^2	0.06	0.12	0.13	0.03
F	12.65	26.73	29.16	5.27
	Event Period Interval			
	5	6	7	8
$\hat{a}_{1t}$	-0.011	-0.000	-0.001	-0.002
$t(\hat{a}_{1t})$	-4.88	-0.15	-0.59	-0.69
$\hat{a}_{2t}$	-0.005	-0.012	-0.007	-0.004
$t(\hat{a}_{2t})$	-2.75	-4.45	-4.08	-2.21
$\hat{a}_{3t}$	-0.002	-0.008	0.000	0.006
$t(\hat{a}_{3t})$	-1.65	-3.48	0.15	3.48
R^2	0.03	0.09	0.04	0.04
F	5.79	18.21	8.39	7.65

Table 4-7 (continued)

Event Periods 9-11

	Event Period Interval		
	9	10	11
$\hat{a}_{1t}$	0.002	0.002	0.007
$t(\hat{a}_{1t})$	0.94	1.12	3.56
$\hat{a}_{2t}$	0.001	-0.006	0.003
$t(\hat{a}_{2t})$	0.44	-3.70	1.55
$\hat{a}_{3t}$	-0.004	0.001	-0.001
$t(\hat{a}_{3t})$	-2.90	0.90	-0.62
R^2	0.02	0.03	0.01
F	4.22	6.95	1.29

Table 4-8

Summary of Estimates $\hat{a}_{3t}$ from Cross-Sectional Regression

$$R_{jt} = a_{1t} + a_{2t} \beta_{jt} + a_{3t} Z_j + w_{jt}$$

159 "Non-Event" Weeks

		$\hat{a}_{3t}$	$t(\hat{a}_{3t})$
Quartiles	0 (min)	-0.0168	-3.02
	1	-0.0060	-1.10
	2	-0.0010	-0.18
	3	0.0044	0.72
	4 (max)	0.0389	4.21
Average		-0.0003	-0.11
Std. Deviation		0.0088	1.35
$t(\hat{a}_{3t}) < -1.65$		-	13.8%
$t(\hat{a}_{3t}) > 1.65$		-	8.8%
$ t(\hat{a}_{3t}) > 1.65$		-	22.6%

as defined in Table 4-4, plus and minus three weeks. An estimate $\hat{a}_{3t}$ from one of these regressions can be interpreted as the average abnormal return per week over the corresponding interval. Table 4-7 suggests that lessee equities exhibited significant abnormal price behavior during all event period intervals except those associated with the issuance of the first exposure draft (EP7), the issuance of the second exposure draft (EP10), and the final adoption of SFAS 13 (EP11). And a somewhat laborious comparison of Tables 4-6 and 4-7 indicates that $\hat{a}_{3t}$ is largest in magnitude for those event period intervals which include a significant event week.

A straightforward interpretation of the Table 4-7 results is that, after controlling for the common element in security returns, lessee equities tended to perform poorly relative to non-lessee equities during brief periods associated with developments in the lessee accounting standard-setting process. An estimate of abnormal performance over the entire standard-setting process can be obtained by multiplying each event period interval estimate $\hat{a}_{3t}$ by the number of weeks in the corresponding interval, and summing across intervals. For the 82 weeks encompassed by the eleven event period intervals, the rather dramatic result of this procedure is an estimated "cumulative average abnormal return" of -23%.

It is the freely admitted prejudice of the author that it would be preposterous to assert a causal relationship between the lessee accounting activities of the various policy boards and the extreme result just cited. Some support for this position can be seen in Table 4-8, which summarizes the distributions of $\hat{a}_{3t}$ and $t(\hat{a}_{3t})$ for

the 159 "non-event" weeks of the study period. If two assumptions that are important in arriving at the -23% figure are maintained -- namely, that returns are generated by a one factor process and that the list of events given in Table 4-3 is reasonably comprehensive -- then one would expect $\hat{a}_{3t}$ to behave as a mean-zero random variable during these weeks. One would also expect the statistic

$$t(\hat{a}_{3t}) = \frac{\hat{a}_{3t}}{\hat{s}_{a_{3t}}}$$

to have a t-distribution appropriate for large degrees of freedom. In particular, one should observe $t(\hat{a}_{3t}) < -1.65$ in about 5% of the 159 weeks, and likewise for $t(\hat{a}_{3t}) > 1.65$. While one does observe what is essentially a zero sample mean for $\hat{a}_{3t}$, it can also be seen that "significant" negative (positive) t statistics occur almost three times (twice) as often as expected. The latter feature of the non-event week estimates casts serious doubt on any causal interpretation of the event week estimates themselves.

This profusion of large t statistics could result either from a general upward bias in the magnitude of $\hat{a}_{3t}$, or from a downward bias in estimated standard error, or both. The former type of bias, which is especially critical if one's object is to arrive at a quantitative assessment of the "true" extent of abnormal returns to lessee shares during the standard setting process, could arise either because the cross-sectional model (9) is misspecified, or because of other defects in the estimation procedure. Thus, it is appropriate at this point to consider several econometric problems connected with the estimation of (9).

Econometric Problems

The estimated coefficients, standard errors, t statistics, etc. produced by the procedure just outlined can be accepted at face value only if the data conform to a number of standard assumptions. Most importantly, the u_{jt} of (6) and the w_{jt} of (9) must be homoskedastic, cross-sectionally independent, and uncorrelated with the regressor variables. Under these conditions, $\hat{a}_{1t}$, $\hat{a}_{2t}$, and $\hat{a}_{3t}$ from (9), and the corresponding estimated standard errors, will be unbiased when sample sizes are large. There is some reason to believe, however, that some or all of the above-mentioned assumptions are actually violated in the present situation. The violations, and their potential effects on the results reported above, are now discussed.

1. Heteroskedasticity If the variance of w_{jt} in (9) is not constant across observations, and if it is assumed that other departures from "standard" regression assumptions are negligible, then the OLS coefficient estimates will be unbiased and consistent (Kmenta, 1971). However, if $\hat{\beta}_j$ and Z_j are positively associated with $\text{var}(w_{jt})$, the estimated standard errors of the coefficient estimates will be biased downward, leading to a tendency to incorrectly reject the null hypothesis that $a_{3t} = 0$.

2. Random Error in the Measurement of β_j As indicated above, the measured value of the regressor β_j in (9) is an estimate $\hat{\beta}_j$ derived from equation (10). If β_j is stationary, as has been tentatively

assumed, then $\hat{\beta}_j$ is unbiased but includes an element of measurement error:

$$\hat{\beta}_j = \beta_j + v_j, \quad (12)$$

and (9) can be more accurately rewritten as

$$R_{jt} = a_{1t} + a_{2t}\hat{\beta}_j + a_{3t}Z_j + (w_{jt} - a_{2t}v_j). \quad (13)$$

It can be seen that the measured regressor $\hat{\beta}_j$ and the "revised" disturbance term $(w_{jt} - a_{2t}v_j)$ are contemporaneously correlated, and this in turn implies that the OLS estimates $\hat{a}_{2t}$ and $\hat{a}_{3t}$ will be biased and inconsistent (Johnston, 1972; Levi, 1973).² It can be shown that if the v_j are homoskedastic, independent of β_j , and mutually uncorrelated, and if Z_j is measured without error, then the asymptotic biases in $\hat{a}_{2t}$ and $\hat{a}_{3t}$ are given by:

$$\text{plim}(\hat{a}_{2t}) - a_{2t} = - \frac{\sigma_v^2 \sigma_{33}}{(\sigma_{22}\sigma_{33} - \sigma_{23}^2) + \sigma_v^2 \sigma_{33}} \cdot a_{2t} = M a_{2t} \quad (14a)$$

and

$$\text{plim}(\hat{a}_{3t}) - a_{3t} = \frac{\sigma_v^2 \sigma_{23}}{(\sigma_{22}\sigma_{33} - \sigma_{23}^2) + \sigma_v^2 \sigma_{33}} \cdot a_{2t} = N a_{2t} \quad (14b)$$

where the σ_{ij} are elements of the (limiting) covariance matrix of "true" regressors, and σ_v^2 is the error variance.

By the Schwartz inequality, $(\sigma_{22}\sigma_{33} - \sigma_{23}^2) > 0$; thus the denominators of both M and N are positive. M can be seen to lie in the interval $(-1, 0)$, indicating that, as an estimate of a_{2t} , $\hat{a}_{2t}$ is

in the limit biased toward zero. The sign of N depends on the sign of $\sigma_{23} = \text{cov}(\beta_j, Z_j)$ which, as the data in Table 4-5 suggest, is probably positive in the present context. This means that $\hat{a}_{3t}$ will tend to be biased downward (upward) when the true value of a_{2t} is negative (positive).

In the one factor world that has been assumed, $a_{2t} = b_{mt}(\alpha_{2t} - \phi_t)$. Because a large proportion of the events examined in this study occurred during what was, ex post, a lengthy bear market, it can be conjectured (and the data of Tables 4-7 and 4-8 suggest) that the true value of a_{2t} was frequently negative during the study period. Therefore, one would expect $\hat{a}_{3t}$ to exhibit a tendency toward negative bias, and thus to indicate "negative abnormal returns" to lessee shares where none may in fact have existed.

The importance of such bias can be guessed at using data already at hand. First, note that equations (14) can be consolidated and rewritten as

$$\text{plim}(\hat{a}_{3t}) - a_{3t} = \frac{N}{1+M} \cdot \text{plim}(\hat{a}_{2t}) \quad (15)$$

If values from the sample covariance matrix of regressors are used to approximate σ_{22} , σ_{33} , and σ_{23} , and if the sample average value of S_{β}^2 is used to approximate σ_v^2 , then

$$\hat{\sigma}_{22} = 0.175693$$

$$\hat{\sigma}_{33} = 0.250633$$

$$\hat{\sigma}_{23} = 0.026569$$

$$\hat{S}_{\beta}^2 = 0.059074$$

$$\hat{M} = -0.2547$$

$$\hat{N} = 0.0270$$

and

$$\text{plim}(\hat{a}_{3t}) - a_{3t} = 0.0362 \text{ plim}(\hat{a}_{2t}). \quad (16)$$

The extent of bias in $\hat{a}_{3t}$ due to random measurement error can now be assessed by substituting actual estimates $\hat{a}_{2t}$ for the corresponding probability limits in (16). Looking back at Tables 4-6 and 4-7, it appears that such bias is probably quite small: for event weeks 1-16 and event periods 1-11, $|\hat{a}_{2t}|$ falls almost without exception in the range (0.00, 0.02), which leads one to conclude that the absolute value of the bias generally does not exceed 0.001 (corresponding to an "abnormal return" of one tenth of one percent).

3. Bias in the Measurement of β_j As indicated in Chapter 2, there is some a priori reason to expect that, as estimates of β_j during the study period, the pre-study period $\hat{\beta}_j$ from (10) may be biased downward, and that the absolute value of the bias may be greater for lessee firms than for non-lessee firms. In the vernacular of Chapter 2, this is the "non-stationarity problem." The presence of such a differential measurement bias may result in misleading estimates of

a_{3t} even if measurements error of the type discussed in the preceding section is completely absent.

In order to get a qualitative idea of the effect of differential measurement bias on the estimated value of a_{3t} , suppose that for every sample firm, $\hat{\beta}_j$ from regression (10) is an exact measurement of the pre-study period β_j . Further suppose that $\hat{\beta}_j$ bears the following relationship to β_{jt} during a given week (or sequence of weeks) during the study period:

$$\hat{\beta}_j = \beta_{jt} + v_j, \quad (17)$$

$$v_j = \begin{cases} 0 & j \in NL \\ v & j \in L, \end{cases}$$

Assuming that all other departures from standard regression assumptions are negligible, it can be shown (see Appendix) that

$$\begin{aligned} \text{plim}(\hat{a}_{2t}) - a_{2t} &= \frac{\sigma_{33}(\sigma_{23}v - k^2v^2)}{(\sigma_{22}\sigma_{33} - \sigma_{23}^2) - 2\sigma_{23}\sigma_{33}v + \sigma_{33}k^2v^2} \cdot a_{2t} \\ &= C a_{2t} \end{aligned} \quad (18a)$$

and

$$\begin{aligned} \text{plim}(\hat{a}_{3t}) - a_{3t} &= \frac{-(\sigma_{22}\sigma_{33}v - \sigma_{23}kv^2)}{(\sigma_{22}\sigma_{33} - \sigma_{23}^2) - 2\sigma_{23}\sigma_{33}v + \sigma_{33}k^2v^2} \cdot a_{2t} \\ &= Da_{2t}, \end{aligned} \quad (18b)$$

where the σ_{ij} are elements of the covariance matrix of "true" regressors, k is the ratio of lessee observations to total observations, and v is as defined in (17).

In this simplified example, v represents the relative measurement bias, at time t , in the pre-study period $\hat{\beta}_j$ of lessee firms. If, in accordance with the argument of Chapter 2, v is taken to be a negative number, and if $\sigma_{23} = \text{cov}(\beta_{jt}, z_j)$ is again taken to be positive, then the signs of C and D can be determined. C is negative, indicating that, in the limit, $\hat{a}_{2t}$ is biased toward zero. D is positive, suggesting that $\hat{a}_{3t}$ will tend to be biased negatively (positively) when the true value of $\hat{a}_{2t}$ is negative (positive).

The implication of equations (18) for statistical inference are thus similar to those discussed in connection with the case of pure random measurement error: if the true value of a_{2t} were frequently negative during the study period, one would again expect a tendency for $\hat{a}_{3t}$ to be negatively biased, and therefore to overstate (understate) the magnitude of real negative (positive) abnormal returns to lessee shares. The opposite result would hold if the true value of a_{2t} were frequently positive. This is, in fact, the same judgement that was reached in Chapter 2 when the "non-stationarity problem" was discussed in the context of residual analysis.

Finally, no attempt is made at this point to gauge the extent of the possible bias in $\hat{a}_{3t}$ resulting from differential bias in the measurement of β_{jt} , first because no data has yet been presented to suggest the magnitude of the latter, and second because the model represented by equation (17) is probably too overly simplified to permit a reliable numerical assessment.

4. The Problem of Omitted Variables Perhaps the most crucial assumption underlying regressions (9) is that realized security returns are adequately specified by equation (2). If returns are generated by multiple common factors, (9) must be interpreted as excluding relevant explanatory variables, and bias in estimates of $\hat{a}_{3t}$ is virtually guaranteed. To illustrate this point, suppose that returns are generated by two independent factors with unanticipated components $\tilde{\phi}_{1t}$ and $\tilde{\phi}_{2t}$, each of which has zero mean and unit variance. In this case, equations (2) and (4) can be rewritten (omitting subscript t) as

$$R_j = E_j + b_{j1}\phi_1 + b_{j2}\phi_2 + u_j, \quad (19)$$

and

$$R_j = \alpha_0 + (\alpha_1 + \phi_1)b_{j1} + (\alpha_2 + \phi_2)b_{j2} + u_j. \quad (20)$$

Under the stated conditions, β_j is a linear combination of b_{j1} and b_{j2} . Specifically,

$$\beta_j = \text{cov}(R_j, R_m) = \frac{b_{j1}b_{m1} + b_{j2}b_{m2}}{b_{m1}^2 + b_{m2}^2} \quad (21)$$

Solving (21) for b_{j1} and substituting the result in (20) gives

$$R_j = \alpha_0 + \frac{(\alpha_1 + \phi_1)(b_{m1}^2 + b_{m2}^2)}{b_{m1}} \beta_j + \frac{(\alpha_2 + \phi_2)b_{m1} - (\alpha_1 + \phi_1)b_{m2}}{b_{m1}} b_{j2} + u_j, \quad (22)$$

or, more simply,

$$R_j = a_0 + a_1\beta_j + a_2b_{j2} + u_{jt}. \quad (23)$$

This means that, when two factors generate returns, a correct specification of regression (9) is

$$R_{jt} = a_{0t} + a_{1t}\beta_j + a_{2t}b_{j2} + a_{3t}Z_j + w_{jt}. \quad (24)$$

If b_{j2} is inappropriately excluded from the regression, the asymptotic bias in $\hat{a}_{3t}$ is given by

$$\text{plim}(\hat{a}_{3t}) - a_{3t} = a_{2t} \frac{\text{cov}(b_{j2}, Z_j) \text{var}(\beta_j) - \text{cov}(b_{j2}, \beta_j) \text{cov}(\beta_j, Z_j)}{\text{var}(\beta_j) \text{var}(Z_j) - (\text{cov}(\beta_j, Z_j))^2} \quad (25)$$

Recalling that the pre-study period estimate of $\text{cov}(\beta_j, Z_j)$ was numerically small, one might hazard a guess that the second term in the numerator of the bracketed quotient is close to zero. $\text{var}(\beta_j)$, on the other hand, is relatively large, which suggests that, in a two factor world, the magnitude of the "omitted variables bias" in $\hat{a}_{3t}$ will be very sensitive to $|\text{cov}(b_{j2}, Z_j)|$, or, in different terms, to how well Z_j proxies for b_{j2} .

More generally, if returns are generated by $k + 1$ common factors, the asymptotic bias in $\hat{a}_{3t}$ from (9) can be expressed as a sum of k terms, each of which, like the RHS of (24), is the product of the true value of the coefficient of one omitted factor (i.e., a_{kt}) and a linear combination of the covariances of that factor with the included variables β_j and Z_j . Thus, a potential for serious bias exists if Z_j is strongly correlated with b_{jk} for at least one omitted factor.

Beyond this rather unhelpful observation, it is difficult to say much that is constructive regarding the ultimate effect on $\hat{a}_{3t}$ of excluding relevant factors from the analysis. While the empirical finance literature (e.g., Ross and Roll, 1980) suggests that security returns behave as if generated by a small number of common factors it has, at this point in time, little that is definitive to say about the actual number of such factors or about their successive importance in explaining the cross-sectional variation in returns. The theoretical finance literature, as exemplified by the work of Ross (1977) has shown that when multiple relevant factors are assumed to exist, ex ante expected returns can be modelled in a form that is analogous to the Sharpe-Lintner CAPM, and ex post returns can be modelled in a form that is analogous to equations (4) and (20). But very little attention is given in this literature to the substantive character of such multiple factors. In short, so little is known about the number and nature of the common factors affecting returns, and of the determinants of the sensitivities of individual securities to their behavior, that neither the direction nor the magnitude of the potential "omitted variables" bias in $\hat{a}_{3t}$ can be specified a priori.³

Revised Regression Procedure

The preceding discussion can be summarized in a few words. First, if the one factor process assumed in equation (2) is sufficient as a description of security returns, then the estimated coefficients reported earlier are subject to potential biases stemming from the inaccurate measurement of β_{jt} , and their estimated standard errors are

potentially biased due to heteroskedasticity. The direction (and, to a lesser extent, the magnitude) of these biases is predictable. Second, if the one factor process of (2) is not sufficient as a description of security returns, the estimated coefficients are subject to additional biases resulting from the omission, in equation (9), of "relevant explanatory variables." Biases of the latter type are unpredictable both as to direction and magnitude. The remainder of this section outlines a revised estimating procedure which retains the one factor assumption, but in which reasonable steps are taken to remove the effects of biases associated with heteroskedasticity, pure random error in the measurement of β_{jt} , and differential lessee/non-lessee bias in the measurement of β_{jt} . For a given event week or event period, the final $\hat{a}_{3t}$ obtained using the revised procedure will be interpreted as an unbiased estimate of the average "abnormal return" to lessee firms during that week or period. Unfortunately, this interpretation must be hedged due to the remaining possibility of a "missing factor bias."

The first step in the revised procedure attempts to control for differential measurement bias in β_{jt} . For each sample firm, new "market model" estimates of β_{jt} are obtained which are as local in time as possible with respect to the events being studied. Regression (9) is then rerun for each event week and event period interval, substituting the appropriate local estimates for the pre-study period estimates which were used initially. The intuitive justification for this procedure can be simply stated: if β_j is truly non-stationary, OLS estimates which are very local with respect to the point in time

at which one wishes to know its value should be less biased, on average, then estimates based on data from a period which is far removed.

Details concerning the estimation of local β_{jt} are given in Table 4-9. Because many of the events considered in this study are closely spaced in time, insufficient degrees of freedom are available for a separate local estimate corresponding to each event. As a compromise measure, seven local $\hat{\beta}_{jt}$ are estimated for each sample firm, each based on 28-32 return observations.

The second step in the revised procedure attempts to control for pure random measurement error in local estimates of β_{jt} . As indicated in the preceding section, the presence of such error tends to bias $\hat{a}_{2t}$ in (9) toward zero, and to bias $\hat{a}_{3t}$ negatively (positively) when the sign of a_{2t} is negative (positive). It was also concluded that the magnitude of the "errors-in-variables" bias in $\hat{a}_{3t}$ was probably negligible. When the assumption of β_j -stationarity is dropped, and local estimates of β_j are substituted in (9), this conclusion may no longer be valid. There are two reasons for this. First, since local β_j are estimated with relatively few degrees of freedom, their efficiency is likely to be quite low. In terms of equations (14), the error variance σ_v^2 may be substantially larger than was originally contemplated. Second, if estimation based on pre-study period data truly results in a differential lessee/non-lessee bias in the measurement of β_{jt} , one may expect σ_{23} in equations (14) (i.e., $\text{cov}(\beta_j, Z_j)$) to be larger than the pre-study period data suggest. Together, these differences have the effect of increasing the quotient

Table 4-9
Summary of Weekly Return
Observations Used in Estimating
"Local" β_j

"Local" Estimate	Weeks Preceding	Event Period Interval	Weeks Following	Event Period Interval	n	Events Included
$\hat{\beta}_{j1}$	19	1	9	3	28	1,2,3,4
$\hat{\beta}_{j2}$	9	4	23	4	32	5,6
$\hat{\beta}_{j3}$	21	5	11	5	32	7,8
$\hat{\beta}_{j4}$	12	6	20	6	32	9,10
$\hat{\beta}_{j5}$	15	7	17	8	32	11,12
$\hat{\beta}_{j6}$	21	9	11	10	32	13,14,15
$\hat{\beta}_{j7}$	11	11	19	11	30	16

$N/(1+M)$ in equation (15), suggesting at least the possibility of a significant "errors-in-variables" bias in $\hat{a}_{3t}$.

In order to control for this possibility, a standard "error-reduction" method from the finance literature (e.g., Fama and MacBeth, 1973) is employed. Within lessee and non-lessee subsamples, firms are ranked according to pre-study period estimates of β_j . Based on these rankings, each subsample is divided into portfolios of n securities each. For each such portfolio, seven "local" market model estimates $\hat{\beta}_{pt}$ are obtained from the returns series detailed in Table 4-9. Regression (9) is then rerun for every event week and event period interval, in each case using the appropriate "local" estimate $\hat{\beta}_{pt}$ as a regressor.

The effects of this procedure are twofold. First, the standard error of $\hat{\beta}$ is proportional to the standard deviation of the "market model" (i.e., equation (10)) disturbance term and, assuming cross-sectional independence, the latter should itself tend to vary inversely with the square root of n , the portfolio size. Thus, measurement error in $\hat{\beta}$ should generally be smaller at the portfolio level than at the single security level. By moving from a single security version of (9) to a version based on portfolios of reasonable size, one can therefore hope to reduce the level of errors-in-variables bias to manageable proportions. Second, the grouping procedure employed should help to preserve a sufficient "spread" in $\hat{\beta}_{pt}$ to guard against extreme losses in efficiency when estimating (9).

This grouping and re-estimation procedure is performed first using 5-security portfolios, and then repeated using 10-security portfolios. In order to insure that all portfolios include the correct number of securities, it is necessary in the former case to drop two firms from the lessee subsample, and in the latter case to drop seven lessee and five non-lessee firms. Firms to be dropped are chosen at random. At the 5- and 10-security levels, respectively, 75 and 35 degrees of freedom are available for estimating the coefficients of (9). While using larger portfolios would unreasonably reduce the number of degrees of freedom available for estimation, substantial reductions in measurement error should be achievable even at these levels. For example, at the 10-security level one would expect the average measurement error in $\hat{\beta}$ to be about one third (actually $1/\sqrt{10}$) of that at the one security level.

A final benefit of the grouping procedure just outlined is that, at the portfolio level, one would expect a decrease in the range of the variance of w_{jt} . That is, the departure from the assumption of homoskedasticity should be less radical at the portfolio level than at the single security level, and therefore bias in the estimated standard error of $\hat{a}_{3t}$ should be somewhat diminished.

To summarize briefly, the revised estimation procedure can be expressed, for a given event week or event period interval t , as a sequence of three regression equations:

$$\text{I. } R_{jt} = a_{1t} + a_{2t} \hat{\beta}_{jt}^* + a_{3t} Z_j + w_{jt} \quad j = 1, \dots, 392 \quad (25a)$$

$$\text{II. } R_{pt} = a_{1t} + a_{2t} \hat{\beta}_{pt}^* + a_{3t} Z_p + w_{pt} \quad p = 1, \dots, 78 \quad (25b)$$

$$\text{III. } R_{pt} = a_{1t} + a_{2t} \hat{\beta}_{pt}^* + a_{3t} Z_p + w_{pt} \quad p = 1, \dots, 38 \quad (25c)$$

where $\hat{\beta}_{.t}^*$ is the "local" estimate appropriate for period t . Each step in the revised procedure is intended to make an incremental contribution toward reducing potential biases inherent in the preliminary estimates $\hat{a}_{3t}$ reported previously. Regression I is designed to eliminate bias in $\hat{a}_{3t}$ due to differential measurement bias in pre-study period lessee and non-lessee β -estimates. Regressions II and III are intended to further control (to the extent allowed by the overall sample size) for remaining biases of the pure "errors-in-variables" type. Finally, it is expected that as one progresses from I to III, biases in the estimated standard error of $\hat{a}_{3t}$ due to the presence of an heteroskedastic disturbance term will also be reduced.

Revised Results

A summary of "local" market model β estimates, their estimated standard errors, and residual variances for the single security, 5-security, and 10-security portfolio levels is provided in Tables 4-10, 4-11, and 4-12. In each table, a summary of "prior" estimates derived from pre-study period return data is also provided for comparison purposes.

The sample average statistics presented in Table 4-10 suggest that the difference between lessee and non-lessee β 's tended to be larger during the study period than during the pre-study period. During the first and second local estimation periods in particular, the difference between average lessee and non-lessee estimates is roughly twice that which obtained during the pre-study period.

Table 4-10
Distribution of "Local" β -Estimates - Summary Statistics

Portfolio Size (Sample Size)	Prior	Local							
		1		2		3			
		L	NL	L	NL	L	NL		
1 (392)	min.	0.24	0.11	-0.15	-0.78	0.05	0.04	-0.24	-0.62
	max.	2.08	2.20	3.05	2.37	2.33	2.29	3.08	2.59
	avg.	1.05	0.95	1.09	0.87	1.06	0.88	1.04	0.98
	s.d.	0.41	0.42	0.57	0.54	0.46	0.42	0.51	0.54
5 (78)	min.	-	-	0.48	0.34	0.58	0.50	0.54	0.56
	max.	-	-	1.96	1.73	1.60	1.45	1.80	1.62
	avg.	-	-	1.09	0.86	1.06	0.88	1.04	0.98
	s.d.	-	-	0.37	0.31	0.24	0.22	0.25	0.24
10 (38)	min.	-	-	0.69	0.47	0.72	0.61	0.81	0.62
	max.	-	-	1.62	1.64	1.45	1.31	1.51	1.30
	avg.	-	-	1.09	0.87	1.07	0.89	1.04	0.99
	s.d.	-	-	0.29	0.27	0.20	0.18	0.19	0.16

Table 4-10 (continued)

Portfolio Size (Sample Size)	Local									
	4		5		6		7		7	
	I.	NL	L	NL	L	NL	L	NL	L	NL
1 (392)	min.	-0.04	-0.90	-0.38	-0.17	-0.11	0.01	-1.67	-1.01	
	max.	2.60	2.48	3.72	2.33	4.82	2.74	3.79	3.43	
	avg.	1.02	0.87	1.05	0.89	1.03	0.88	1.05	1.04	
	s.d.	0.47	0.45	0.57	0.45	0.63	0.50	0.76	0.63	
5 (78)	min.	0.55	0.43	0.40	0.39	0.37	0.50	0.47	0.42	
	max.	1.81	1.29	2.06	1.39	2.44	1.52	2.02	1.83	
	avg.	1.02	0.87	1.05	0.89	1.03	0.88	1.05	1.04	
	s.d.	0.27	0.20	0.37	0.25	0.42	0.29	0.35	0.30	
10 (38)	min.	0.74	0.60	0.46	0.56	0.43	0.57	0.59	0.62	
	max.	1.61	1.25	1.81	1.36	2.00	1.38	1.65	1.56	
	avg.	1.02	0.89	1.05	0.90	1.03	0.89	1.06	1.05	
	s.d.	0.24	0.16	0.31	0.23	0.36	0.27	0.27	0.24	

Table 4-11
Estimated Standard Errors of "Prior" and "Local" β -Estimates

Portfolio Size (Sample Size)	Prior	Local				
		1	2	3	4	5 6 7
1 (392)	min.	0.09	0.11	0.10	0.08	0.15 0.13 0.18
	max.	0.54	0.81	0.78	0.86	1.47 1.42 1.73
	avg.	0.23	0.40	0.28	0.28	0.42 0.39 0.59
5 (78)	min.	-	0.09	0.07	0.08	0.10 0.08 0.16
	max.	-	0.31	0.24	0.22	0.26 0.46 0.53
	avg.	-	0.18	0.14	0.13	0.14 0.20 0.19 0.28
10 (38)	min.	-	0.08	0.07	0.06	0.07 0.08 0.06 0.04
	max.	-	0.25	0.15	0.14	0.15 0.27 0.25 0.42
	avg.	-	0.13	0.10	0.09	0.10 0.15 0.13 0.20

Table 4-12

Variance of Market Model Disturbance Term
"Prior" and "Local" Estimates

Portfolio Size (Sample Size)		Prior	Local							
			1	2	3	4	5	6	7	
1 (392)	max./									
	min.	38.22	96.04	59.61	58.62	117.24	101.18	119.61	90.45	91.82
	Q3/Q1	2.94	3.03	2.88	2.59	2.82	2.82	3.22	3.01	2.91
	avg.	0.0025	0.0028	0.0040	0.0038	0.0052	0.0041	0.0030	0.0020	0.0036
10 (38)	max./									
	min.	-	11.28	4.00	4.65	4.80	12.70	17.74	9.17	9.19
	Q3/Q1	-	2.07	1.96	1.88	1.90	1.83	2.04	2.02	1.96
	avg.	-	0.0003	0.0004	0.0004	0.0006	0.0004	0.0003	0.0002	0.0004

Table 4-11 indicates that when β is estimated locally at the single security level, the measurement error involved is simply enormous. For example, in the first local estimation period, the average standard error is 0.40, correspondingly to a 95% confidence interval with a range of roughly 1.60. This general lack of precision can be attributed partly to the fact that the variance of the market model disturbance term appears to be relatively large during most local periods, and partly to local variations in the spread of R_{mt} . The latter, which affects the denominator of the standard error calculation, was relatively large during local periods 2, 3 and 4, relatively small during periods 1, 5 and 6, and quite small during period 7. The extreme magnitude of the measurement error at the single security level suggests that "errors-in-variables bias" may indeed be a problem in connection with cross-sectional regression I.

It can also be seen from Table 4-11 that as one progresses from the single security level to the 10-security portfolio level, a substantial reduction in standard error is achieved. In each local estimation period, the average 10-security standard error is roughly one-third of the average single security standard error, and the maximum 10-security standard error is 17-25% of the maximum single security standard error. Thus, one would expect, through regressions II and III, to obtain estimates of a_{3t} which are less biased than those produced at the single security level.

Finally, if the estimated variance of the market model disturbance term is interpreted as a proxy for $\text{var}(w_t)$ in regressions I-III, Table 4-12 suggests that heteroskedasticity may be much less a

problem at the portfolio level than at the single security level. Over the seven local estimation periods, the average ratio of maximum estimated variance to minimum estimated variance is, at the single security level, about 91.82. At the 10-security portfolio level, this ratio drops, on average, to about 9.19. The ratio of the limits of the interquartile range (i.e., $Q3/Q1$) also declines on average from 2.91 to 1.96. On the basis of these numbers, one can argue that, while the data still do not conform to the standard regression assumption of homoskedasticity, standard errors and t-statistics computed at the 10-security level should nevertheless be more reliable than those computed at the single security level.

Estimated coefficients $\hat{a}_{1t}$, $\hat{a}_{2t}$, and $\hat{a}_{3t}$ from regressions I-III are presented in Tables 4-13 (for event weeks) and 4-14 (for event period intervals). For comparison purposes, corresponding "preliminary" estimates (i.e., those obtained using pre-study period or "prior" β -estimates) are also included in each table. Estimated standard errors and t statistics for all event period interval estimates $\hat{a}_{3t}$ are given in Table 4-15.

The following observations can be made by way of summarizing the large mass of data included in Tables 4-13 and 4-14:

1. In 12 of 16 event weeks and in 8 of 11 event period intervals, the effect of moving from the preliminary form of the cross-sectional regression (based on pre-study period β -estimates and 392 individual equities) to the final form

Table 4-13
Revised Regression Results
- Event Weeks -

Coefficient	Regression	df	Event Week					
			1	2	3	4	5	6
$\hat{a}_{1t}$	Prior	389	-0.0115*	-0.0197*	0.0094	-0.0177*	0.0169*	0.0214*
	I	389	-0.0085	-0.0378*	0.0134*	0.0010	0.0285*	0.0301*
	II	75	-0.0076	-0.0183*	-0.0038	-0.0062	0.0012	0.0226*
	III	35	-0.0090	-0.0047	0.0013	-0.0171	-0.0127	0.0137
$\hat{a}_{2t}$	Prior	389	-0.0113*	-0.0494*	0.0040	0.0171*	0.0285*	0.0020
	I	389	-0.0158*	-0.0332*	-0.0002	-0.0029	0.0175*	-0.0077
	II	75	-0.0169*	-0.0559*	0.0197*	0.0055	0.0487*	0.0008
	III	35	-0.0154*	-0.0703*	0.0132	0.0169	0.0644*	0.0109
$\hat{a}_{3t}$	Prior	389	-0.0094*	0.0006	-0.0047	-0.0110*	-0.0060	-0.0239*
	I	389	-0.0071	0.0026	-0.0043	-0.0085	-0.0062*	-0.0223*
	II	75	-0.0070	0.0084	-0.0095	-0.0097	-0.0119*	-0.0227*
	III	35	-0.0084*	0.0108	-0.0079	-0.0126*	-0.0150*	-0.0246*

* $|t| > 1.65$

Table 4-13 (continued)

Coefficient	Regression	df	Event Week							
			7	8	9	10	11	12		
$\hat{a}_{1t}$	Prior	389	-0.0321*	-0.0117	-0.0352*	0.0122	0.0302*	0.0101		
	I	389	-0.0186*	-0.0197*	-0.0248*	0.0064	0.0332*	0.0152*		
	II	75	-0.0079	-0.0133	-0.0164*	0.0288*	0.0362*	0.0150*		
	III	35	0.0024	-0.0272*	-0.0049	0.0301*	0.0369*	0.0181		
$\hat{a}_{2t}$	Prior	389	-0.0042	-0.0034	-0.0050	-0.0130*	0.0005	-0.0007		
	I	389	-0.0229*	0.0049	-0.0176*	-0.0074	-0.0032	-0.0065		
	II	75	-0.0287*	-0.0017	-0.0274*	-0.0331*	-0.0061	-0.0063		
	III	35	-0.0388*	0.0121	-0.0396*	-0.0352*	-0.0070	-0.0088		
$\hat{a}_{3t}$	Prior	389	-0.0010	0.0064	-0.0104*	-0.0004	0.0068	0.0096*		
	I	389	-0.0001	0.0058	-0.0073	-0.0046	0.0076	0.0106*		
	II	75	-0.0003	0.0068	-0.0068	-0.0009	0.0071	0.0109*		
	III	35	0.0001	0.0057	-0.0047	-0.0007	0.0079	0.0101		

* $|t| > 1.65$

Table 4-13 (continued)

Coefficient	Regression	df	Event Week					Avg R^2
			13	14	15	16	a	
$\hat{a}_{1t}$	Prior	389	-0.0009	-0.0000	-0.0004	-0.0055	-0.0345	-0.0022
	I	389	-0.0037	-0.0085	-0.0092*	-0.0022	-0.0052	-0.0003
	II	75	0.0027	-0.0029	0.0012	0.0043	0.0356	0.0022
	III	35	-0.0010	0.0048	-0.0011	-0.0119	0.0177	0.0011
$\hat{a}_{2t}$	Prior	389	-0.0110*	-0.0136*	-0.0141*	0.0043	-0.0693	-0.0043
	I	389	-0.0085*	-0.0049	-0.0052	0.0009	-0.1127	-0.0070
	II	75	-0.0158*	-0.0112	-0.0168*	-0.0056	-0.1508	-0.0094
	III	35	-0.0124*	-0.0203*	-0.0144*	0.0096	-0.1351	-0.0084
$\hat{a}_{3t}$	Prior	389	0.0045	0.0079	-0.0006	0.0055	-0.0261	-0.0016
	I	389	0.0045	0.0071	-0.0014	0.0058	-0.0178	-0.0011
	II	75	0.0057	0.0081	0.0001	0.0064	-0.0147	-0.0009
	III	35	0.0056	0.0094*	-0.0001	0.0058	-0.0186	-0.0012

* $|t| > 1.65$

Table 4-14

Revised Regression Results
- Event Period Intervals -

Coef- ficient	Regression	df	Event Period Interval						
			1	2	3	4	5	6	7
$\hat{a}_{1t}$	Prior	389	-0.0054*	-0.0032	-0.0043*	0.0094*	-0.0105*	-0.0004	-0.0012
	I	389	-0.0031*	-0.0099*	-0.0052*	0.0176*	-0.0038*	0.0032	-0.0005
	II	75	-0.0031	-0.0050*	-0.0018	0.0102*	-0.0018	0.0122*	0.0044*
	III	35	-0.0036	0.0012	0.0008	0.0092*	-0.0031	0.0195*	0.0034
$\hat{a}_{2t}$	Prior	389	-0.0059*	-0.0139*	-0.0103*	0.0007	-0.0053*	-0.0120*	-0.0074*
	I	389	-0.0092*	-0.0074*	-0.0102*	-0.0085*	-0.0120*	-0.0171*	-0.0087*
	II	75	-0.0092*	-0.0131*	-0.0142*	0.0000	-0.0140*	-0.0275*	-0.0140*
	III	35	-0.0088*	-0.0199*	-0.0170*	0.0013	-0.0127*	-0.0357*	-0.0130*
$\hat{a}_{3t}$	Prior	389	-0.0062*	-0.0033*	-0.0060*	-0.0057*	-0.0027*	-0.0079*	0.0002
	I	389	-0.0048*	-0.0031*	-0.0048*	-0.0041*	-0.0025*	-0.0068*	0.0007
	II	75	-0.0045*	-0.0018	-0.0037*	-0.0056*	-0.0025*	-0.0052*	0.0015
	III	35	-0.0048*	-0.0007	-0.0035*	-0.0060*	-0.0028*	-0.0039*	0.0012

* $|t| > 1.65$

Table 4-14 (continued)

Coef- ficient	Regression	df	Event Period Interval						
			8	9	10	11			
$\hat{a}_{1t}$	Prior	389	-0.0015	0.0019	0.0020	0.0072*	-0.0458	-0.0006	
	I	389	-0.0000	0.0040*	-0.0013	0.0120*	0.1015	0.0012	
	II	75	0.0018	0.0042*	0.0004	0.0091*	0.2334	0.0029	
	III	35	0.0006	0.0019	0.0009	0.0056*	0.2821	0.0034	
$\hat{a}_{2t}$	Prior	389	-0.0043*	0.0008	-0.0059*	0.0028	-0.4577	-0.0056	
	I	389	-0.0062*	-0.0016	-0.0026*	-0.0020*	-0.6489	-0.0079	
	II	75	-0.0083*	-0.0018	-0.0046*	0.0006	-0.8030	-0.0098	
	III	35	-0.0070*	0.0007	-0.0053*	0.0039	-0.8639	-0.0105	
$\hat{a}_{3t}$	Prior	389	0.0057*	-0.0043*	0.0012	-0.0009	-0.2304	-0.0028	0.05
	I	389	0.0062*	-0.0040*	0.0010	-0.0006	-0.1768	-0.0022	0.09
	II	75	0.0065*	-0.0039*	0.0013	-0.0005	-0.1445	-0.0018	0.23
	III	35	0.0061*	-0.0039*	0.0015	-0.0005	-0.1358	-0.0017	0.34

* $|t| > 1.65$

Table 4-15
t-Statistics and Estimated Standard Errors
- Event Period Intervals -

Regression	Event Period Interval					
	1	2	3	4	5	6
$\hat{a}_{3t}$	-0.0062	-0.0033	-0.0060	-0.0057	-0.0027	-0.0079
Prior $t(\hat{a}_{3t})$	-3.67	-1.90	-4.08	-3.25	-1.65	-3.48
$s_{\hat{a}_{3t}}$	0.00169	0.00171	0.00146	0.00177	0.00162	0.00227
$\hat{a}_{3t}$	-0.0048	-0.0031	-0.0048	-0.0041	-0.0025	-0.0068
I $t(\hat{a}_{3t})$	-2.94	-1.75	-3.37	-2.33	-1.69	-3.09
$s_{\hat{a}_{3t}}$	0.00165	0.00178	0.00142	0.00175	0.00149	0.00220
$\hat{a}_{3t}$	-0.0045	-0.0018	-0.0037	-0.0056	-0.0025	-0.0052
II $t(\hat{a}_{3t})$	-2.51	-0.90	-2.35	-3.08	-1.81	-2.22
$s_{\hat{a}_{3t}}$	0.00181	0.00202	0.00159	0.00183	0.00137	0.00233
$\hat{a}_{3t}$	-0.0048	-0.0007	-0.0035	-0.0060	-0.0028	-0.0039
III $t(\hat{a}_{3t})$	-2.31	-0.36	-1.91	-2.89	-1.81	-1.92
$s_{\hat{a}_{3t}}$	0.00207	0.00189	0.00181	0.00208	0.00156	0.00200
$s_{\hat{a}_{3t}}(I11)/s_{\hat{a}_{3t}}(I)$	1.25	1.06	1.27	1.19	1.05	0.91

Table 4-15 (continued)

Regression	Event Period Interval					
	7	8	9	10	11	
Prior	$\hat{a}_{3t}$	0.0002	0.0057	-0.0043	0.0012	-0.0009
	$t(\hat{a}_{3t})$	0.15	3.48	-2.90	0.90	-0.62
	$s_{\hat{a}_{3t}}$	0.00152	0.00164	0.00149	0.00183	0.00152
I	$\hat{a}_{3t}$	0.0007	0.0062	-0.0040	0.0010	-0.0006
	$t(\hat{a}_{3t})$	0.50	3.84	-2.70	0.72	-0.42
	$s_{\hat{a}_{3t}}$	0.00149	0.00162	0.00149	0.00135	0.00150
II	$\hat{a}_{3t}$	0.0015	0.0065	-0.0039	0.0013	-0.0005
	$t(\hat{a}_{3t})$	0.90	4.05	-2.99	0.88	-0.36
	$s_{\hat{a}_{3t}}$	0.00163	0.00161	0.00132	0.00153	0.00136
III	$\hat{a}_{3t}$	0.0012	0.0061	-0.0039	0.0015	-0.0005
	$t(\hat{a}_{3t})$	0.71	3.73	-2.84	0.91	-0.39
	$s_{\hat{a}_{3t}}$	0.00166	0.00163	0.00137	0.00169	0.00139
$s_{\hat{a}_{3t}}^{(III)}/s_{\hat{a}_{3t}}^{(I)}$	1.11	1.01	0.92	1.25	0.93	

(based on local β -estimates and 38 ten-security portfolios)
is to:

- a. make $\hat{a}_{2t}$ more negative (more positive) when the preliminary estimate $\hat{a}_{2t}$ was negative (positive); and
- b. to make $\hat{a}_{3t}$ more positive (negative) when the preliminary estimate of $\hat{a}_{2t}$ was negative (positive).

This general pattern of movement is consistent with the bias and error arguments outlined earlier.

2. If the previously adopted significance criterion is retained (i.e., $|t| > 1.65$), moving from the preliminary to the final form of the cross-sectional regression results in a loss of significance for $\hat{a}_{3t}$ in EW9 and EW12, and a gain of significance in EW5 and EW14. There was no change in the significance of t statistics for the remaining twelve event weeks. With respect to event period intervals, there occurs a loss of significance in EP2, and no change for the remaining 10 intervals.
3. The pattern described in (1) above can also be seen in the behavior of the cumulative and average values of $\hat{a}_{2t}$ and $\hat{a}_{3t}$ presented in the final columns of each table. The overall effect of moving from the preliminary to the final form of the regression is to make $\hat{a}_{2t}$ on average more negative and $\hat{a}_{3t}$ on average more positive. In the case of event period intervals, the estimated "cumulative abnormal return" to

lessees declines in magnitude from -0.2304 in the preliminary regression to -0.1358 in the final form. A similar but less pronounced effect occurs in the case of event weeks.

4. An examination of the "paths" of the cumulative and average values of $\hat{a}_{2t}$ and $\hat{a}_{3t}$ gives the impression that, as one progresses from regression I to regression III, the estimates are converging. Thus, it may not be unreasonable to accept -0.1358 as a relatively unbiased estimate of the true difference between cumulative lessee and non-lessee returns during periods surrounding the accounting events of interest.

5. In order to conclude, on the basis of standard significance tests, that lessee returns differed from non-lessee returns during a given period, one must have not only an unbiased estimate of a_{3t} , but also an unbiased estimate $s_{a_{3t}}^{\wedge}$ of its standard error. As indicated earlier, the values of $s_{a_{3t}}^{\wedge}$ calculated in connection with the various cross-sectional regressions may be biased downward due to the presence of a heteroskedastic disturbance term. This would have the effect of making the $\hat{a}_{3t}$ appear to be more precise than is actually the case, and could possibly result in an incorrect rejection of the null hypothesis of "no difference". Table 4-15, which details estimates $\hat{a}_{3t}$, their estimated standard errors, and the related t statistics for each event period interval,

enables one to say something about the severity of this problem. In eight of the eleven event period intervals, there occurs an increase in estimated standard error as one moves from regression I to regression III. This is what would be expected if, as the data in Table 4-12 indicate, the range of the disturbance variance is being very substantially decreased through application of the grouping procedure. However, the magnitudes of the corresponding increases in $\hat{s}_{a_{3t}}$, ranging from about 1% in the case of EP8 to 27% in the case of EP3, are not in general very dramatic, and this in turn suggests that the t statistics computed for regression III are probably fairly reliable indicators of "statistical significance".

6. The data in Table 4-15 can also be used to construct t statistics and/or confidence intervals for the cumulative estimates $\sum_t n_t \hat{a}_{3t}$ given in Table 4-14. If $\hat{a}_{3t}$ is accepted as an unbiased estimate with expectation a_{3t} and variance σ_a^2 , then the expectation and variance of $\sum_t n_t \hat{a}_{3t}$ are given by

$$E(\sum_t n_t \hat{a}_{3t}) = \sum_t n_t a_{3t} \quad (26)$$

$$\text{var}(\sum_t n_t \hat{a}_{3t}) = \sum_t n_t^2 \sigma_a^2 \quad (27)$$

Under the null hypothesis that $\sum_t n_t a_{3t} = 0$, the statistic

$$t(\sum_t n_t \hat{a}_{3t}) = \frac{\sum_t n_t \hat{a}_{3t}}{(\sum_t n_t^2 s_{a_{3t}}^2)^{1/2}}, \quad (28)$$

where $s_{a_{3t}}^2$ is the square of the estimated standard error of $\hat{a}_{3t}$, has a Student's t distribution with a minimum of 35 degrees of freedom. Similarly, 90% confidence intervals for $\Sigma_t n_t a_{3t}$ are given by

$$\Sigma_t n_t \hat{a}_{3t} \pm 1.65 (\Sigma_t n_t^2 s_{a_{3t}}^2)^{1/2}. \quad (29)$$

Confidence intervals and t statistics computed in this manner for each cross-sectional regression level are shown in Table 4-16.

Several observations can be made concerning the contents of this table. First, $(\Sigma_t n_t^2 s_{a_{3t}}^2)^{1/2}$ increases steadily as one moves from regression I to regression III, again suggesting an overall downward bias in the estimated standard errors of the individual $\hat{a}_{3t}$. However, the slender magnitude of these increases also suggests that, at the regression III level, the practical effect of any remaining bias is probably negligible. Second, as one moves from the preliminary regression to regression III, the 90% confidence interval for $\Sigma_t n_t a_{3t}$ exhibits a positive displacement but shows little changes in size. The positive displacement is consistent with the "measurement bias" and "measurement error" arguments presented earlier in this chapter. The fact that the size of the confidence interval changes very little is simply a reflection of the behavior of $(\Sigma_t n_t^2 s_{a_{3t}}^2)^{1/2}$. Finally, as one moves from the preliminary to the final form of the regression, $t(\Sigma_t n_t \hat{a}_{3t})$ remains "significant" but declines

Table 4-16
Confidence Intervals and t-Statistics for $\Sigma_t n_t a_{3t}$

Regression	"Cum. Abnormal Return" $\Sigma_t n_t \hat{a}_{3t}$	Est. Std. Error $(\Sigma_t n_t^2 S_{a_{3t}}^2)^{\frac{1}{2}}$	90% Confidence Interval	
			$t(\Sigma_t n_t \hat{a}_{3t})$	
Prior	-0.2304	0.0412	-5.59	-0.2984
I	-0.1768	0.0405	-4.38	-0.2436
II	-0.1445	0.0423	-3.42	-0.2143
III	-0.1358	0.0437	-3.12	-0.2079

steadily. In light of the observations just made, it would seem that this decline is due primarily to the elimination of bias and pure random error in the measurement of β_{jt} .

4.6 Interpretation and Discussion

This section opens with a rather strong interpretation of the empirical results just reported, whose validity depends on the correctness of a number of implicit or explicit assumptions which have been made along the way. After this interpretation has been stated, each such assumption is briefly but critically reviewed. Finally, to the extent possible, a comparison of the results reported above with those of other studies is provided.

Interpretation

Given the assumptions implicit and explicit in the empirical analysis, three conclusions are reasonably supported by the results of that analysis.

C1: After controlling for the systematic component in security returns, returns to lessee shares differed significantly (on average) from those to non-lessee shares during as many as four of the sixteen event weeks and seven of the eleven event period intervals under study.

C2: These differences do not appear to have been transitory or self-cancelling. The data indicate that, after controlling

for the systematic component in returns, lessee shares declined in value (relative to non-lessee shares) by about 13.5% during brief periods surrounding the lessee accounting events which occurred during the 1973-1976 period. This "abnormal performance" can be expressed as a probability statement:

the probability that the true value of the cumulative or net difference between average lessee and non-lessee (non-systematic) returns falls between -6% and -21% exceeds 0.90.

C3: The observed differences between lessee and non-lessee returns appear to be related to events connected with the lessee accounting standard-setting process undertaken by the APB, the SEC, and the FASB.

It should be noted in passing that C1 and C2 are merely technical conclusions regarding the presence of a significant difference between the returns to two groups of securities. C3 asserts (or comes close to asserting) a link between this return behavior and the lessee accounting standard-setting process.

In order to arrive at the conclusions just stated, it is extremely important that a number of underlying assumptions be substantially correct. Thus, C1-C3 are intended to be read jointly with the following statement of assumptions.

A1: The process generating returns is sufficiently well specified as a one factor process.

Discussion: If returns are influenced to an important extent by more than one common factor, then, as pointed out earlier, $\hat{a}_{3t}$ may be capturing lessee/non-lessee return differences which are entirely systematic or "expected". In econometric language, $\hat{a}_{3t}$ would be subject to an "omitted variables" bias. Of all the assumptions basic to C1-C3, this one is probably the most crucial: if it is incorrect, then all of the conclusions expressed above may be invalid.

A2: Observed non-systematic return differences are not coincidental.

Discussion: Even if the one factor process assumption is approximately correct, it is still possible to observe significant lessee/non-lessee return differences entirely by coincidence. In order to go from C1 and C2 to C3, it must be assumed that coincidental differences are ruled out. This assumption may well be invalid with respect to a particular event week or event period interval, but it would seem to be reasonable on the "cumulative" level. In the latter instance, return differences associated with sixteen different events are being aggregated, and the coincidental components of such differences can be expected to average out.

A3: The list of events under study is complete.

A4: The "market" does not anticipate an event by more than three weeks.

A5: The market is sufficiently efficient to process the implications of an event into prices within a three week period.

Discussion: These assumptions are necessary to support the conclusion that the lessee/non-lessee return differences associated with the standard-setting process were not transitory or self-cancelling. If any of A3-A5 is violated, the total relevant return difference may not have been adequately captured.

A6: The disturbance terms in the various cross-sectional regressions can be characterized as drawings from a normal distribution.

Discussion: This assumption is sufficient to guarantee the appropriateness of the significance tests on which C1-C3 are based. It is clearly violated when security returns are used as a regressand, since the distribution of returns has a lower bound. However, the assumption may not be necessary. If security returns are "approximately normal" over their usual range, the significance tests and confidence intervals reports earlier should be reasonably reliable. Work by Fama (1976) suggests that "approximate normality"

is probably a fair characterization when returns are calculated in logarithmic price-relative form.

Comparison of Results with those of Previous Studies

The final issue to be raised in this chapter concerns the consistency of the regression III results with results reported in previous studies of the security price impact of lessee accounting standards. The present results can be most feasibly compared to those of the Pfeiffer (1980) study, first because there is a substantial overlap in the set of events considered in the two studies, and second because reasonably detailed quantitative data are available in both cases. In addition, a brief comparison with results from the Leftwich (1981) study of the price impact of APB Opinion No. 16 is also provided.

Some results from the present study and from the Pfeiffer study are presented side by side in Table 4-17. The table includes a list of the event periods examined by Pfeiffer, the length (in weeks) of each period, and the cumulative "risk-adjusted" lessee/non-lessee return difference estimated for each period. Corresponding to each of Pfeiffer's event periods is a group of one or two event period intervals from the present study. The table shows the number, length, and cumulative value of $\hat{a}_{3t}$ for each such interval.

Based on the contents of Table 4-17, the following remarks seem appropriate. First, when return differences are accumulated over all periods common to the two studies, the resulting totals are of essentially the same magnitude. This suggests that a "one-factor"

Table 4-17
Comparison of Results

EP Interval	Present Study		Pfeiffer Study ^c		
	Period ^a Length	Est. Cumulative Return Difference ^b	Est. Cumulative Return Difference	Period Length ^a	Event
2	7	-.0049			APB
3	8	-0.0280*	-0.0367	11	31
4	8	-0.0480*	-0.0036	11	ASR 147
5	8	-0.0224*	-0.0123	11	Disc. Memo.
6	8	-0.0312*	-0.0880*	11	Public Hearings
7	7	0.0084	0.0047	11	ED1
9	7	-0.0273*	-0.0473*	18	ED2
10	8	0.0120			

Table 4-17 (continued)

EP Interval	Present Study		Pfeiffer Study ^c		
	Period ^a Length	Est. Cumulative Return Difference ^b	Est. Cumulative Return Difference	Period Length	Event
11	7	-0.0035	0.0047	11	SFAS 13
Total	68	-0.1449	-0.1488	84	Total

^a in weeks^b based on regression III^c source: Pfeiffer (1980)* $|t| > 1.65$

estimation technique, applied with reasonable care, may well produce the same results (on average) as the more complex and less-well-understood "multiple factor" technique employed by Pfeiffer. Second, on a period by period basis, estimated return differences agree fairly well in terms of algebraic sign, but very poorly in terms of magnitude. Third, for each period in which Pfeiffer found the return difference to be statistically significant, a significant difference was also noted in the present study. There are two plausible explanations for the fact that the converse does not hold.

- (1) Pfeiffer's significance tests are of the form $\bar{d}/(\hat{s}_{\bar{d}}/\sqrt{n})$, where $\bar{d}$ is the average weekly lessee/non-lessee return difference over an event period, $\hat{s}_{\bar{d}}$ is the estimated standard deviation of such differences, and n is the number of weeks in the event period. By computing $\hat{s}_{\bar{d}}$ on the basis of a time series of return differences, Pfeiffer may have controlled for the presence of cross-sectional correlation in returns to an extent not accomplished in the present study, in effect excluding "omitted variables bias" from the statistical results.

- (2) A related possibility arises because Pfeiffer's $\hat{s}_{\bar{d}}$ appears to have been computed using return difference during the event period proper. If return differences were more variable than normal during most event periods, this procedure would induce

an upward bias in $\hat{s}_d^-$ and thus reduce the chance of detecting a significant return difference where one did in fact occur.

A second comparison of interest is one between the present results and those obtained in the Leftwich (1980) study of price movement associated with the APB 16 deliberation process. It will be remembered that Leftwich calculated cumulative "market model" prediction errors for intervals of 2-3 weeks centered on each of 21 APB 16-related events, a period of 261 trading days in all. He found significant abnormal price performance to be associated with 9 of the 21 events. On a cumulative basis over the entire 261 day period, "net" abnormal performance was -11.55%, or, equivalently, about -0.22% per week. This is slightly larger than the average-per-week estimate calculated in the present study (-0.17%, based on the results of regression III), but is nevertheless of the same order of magnitude. This general comparability of the magnitude of abnormal performance reinforces the notion that the lessee accounting standard-setting process of 1973-1976 is potentially useful as a vehicle for studying the contracting effects described in Chapter III.

4.7 Notes

1. Ross's "arbitrage pricing theory" appears to require that: (a) investors are nonsatiating; (b) that they agree on the form of the returns generating process, if not on its parameters; (c) that the number of risky assets is sufficiently large, and their u_i sufficiently independent, to allow the latter to be effectively diversified away; and (d) that short sales be permitted. These assumptions are weak in relation to those required by the Sharpe-Lintner CAPM, for instance.
2. For a discussion of the same problem in connection with cross-sectional tests of the mean-variance CAPM, see Miller and Scholes (1972).
3. Evidence from a number of sources (e.g., Banz (1981)) indicates that firm size may be a good proxy for the sensitivity of a given security to the behavior of one or more "missing" common factors. The "size effect" is not controlled for in the present analysis because the size distributions for lessees and non-lessees do not appear to be substantially different (see Table 4-2). More attention will be devoted to the size effect in the cross-sectional tests described in Chapter 5.

CHAPTER 5
TESTS OF THE
CONTRACTING HYPOTHESIS

5.1 Introduction

It was concluded in Chapter 4 that the behavior of lessee returns during brief intervals surrounding a number of lessee accounting "events" was at least consistent with the hypothesis that prices were adversely affected by the 1973-1976 standard-setting process. The present chapter describes tests which seek to determine the consistency of the data with a particular explanation for such price behavior. This explanation, the "contracting hypothesis," has been described in detail in Chapter 3. Section 5.2 below includes a restatement of the specific hypotheses developed in that chapter. Section 5.3 provides a general description of the regression procedure to be used in testing these hypotheses. Section 5.4 describes sample selection and data collection procedures. Matters relating to the measurement of independent variables are discussed in Section 5.5. The basic test results are presented in Section 5.6, and finally, the "robustness" of these results is assessed in Section 5.7.

5.2 Restatement of Hypotheses

Define u_{jt} as the non-systematic components of the realized return on firm j 's equity in period t . If t can be identified as a period in which the "contracting" effect of a lessee accounting event is impounded in prices, then the nonlinear form of equation (7) in Chapter 3 indicates that the cross-sectional distribution of the u_{jt} should be characterized by a "financial statement impact x leverage" interaction. This in turn suggests that if a sample of firms is partitioned according to "degree of financial statement impact" and "degree of leverage," as shown in Table 5-1, one would expect cell mean values μ_k of the dependent variable u_{jt} to conform to the following system of alternative hypotheses:

$$H_{a1}: \mu_2 - \mu_1 \left\{ \begin{array}{l} < 0 \text{ if the pricing implication of the event} \\ & \text{are negative.} \\ > 0 \text{ if the pricing implications of the event} \\ & \text{are positive.} \end{array} \right.$$

$$H_{a2}: \mu_3 - \mu_1 \left\{ \begin{array}{l} < 0 \text{ if the pricing implications of the event} \\ & \text{are negative.} \\ > 0 \text{ if the pricing implications of the event} \\ & \text{are positive.} \end{array} \right.$$

$$H_{a3}: \begin{array}{l} (\mu_4 - \mu_3) \\ -(\mu_2 - \mu_1) \end{array} \left\{ \begin{array}{l} < 0 \text{ if the pricing implications of the event} \\ & \text{are negative.} \\ > 0 \text{ if the pricing implications of the event} \\ & \text{are positive.} \end{array} \right.$$

The corresponding null hypothesis is, of course, that

$$H_0: \mu_1 = \mu_2 = \mu_3 = \mu_4.$$

Table 5-1
Partitioning Scheme

	Low Leverage	High Leverage
Small Financial Statement Impact	μ_1 (Cell 1)	μ_2 (Cell 2)
Large Financial Statement Impact	μ_3 (Cell 3)	μ_4 (Cell 4)

The discussion in Chapter 3 also suggests that the magnitude of any contracting effect captured in u_{jt} should be moderated by two additional variables. Specifically,

H_{a4}: the magnitude of u_{jt} should vary directly with λ' , the proportion of the firm's debt which is subject to accounting-based restrictions; and

H_{a5}: the magnitude of u_{jt} should vary directly with P, the "publicly traded" proportion of such debt.

5.3 General Description of Test Procedure

The hypotheses outlined in the preceding section will be tested in the following way. The "event period intervals" defined and examined in Chapter 4 will be taken as representing the relevant periods t . For each such event period interval, a cross sectional regression of the following form will be estimated:

$$\hat{u}_{jt} = b_0 + b_1 L_j + b_2 D_j + b_3 L_j D_j + b_4 \text{LSIZE}_j + v_{jt}, \quad (1)$$

where

$$L_j = \begin{cases} 1 & \text{if financial statement impact is potentially "large", and} \\ 0 & \text{otherwise;} \end{cases}$$

$$D_j = \begin{cases} 1 & \text{if leverage is "large," and} \\ 0 & \text{otherwise;} \end{cases}$$

LSIZE_j = a measure of firm size; and

$\hat{u}_{jt}$ = a measure of the nonsystematic component of firm j 's realized equity return in period t .

A regression of this form will also be estimated using a version of $\hat{u}_{jt}$ which is aggregated across all eleven event period intervals.

Specific procedures to be followed in measuring the independent variables included in (1) will be discussed in Section 5.5.

Meanwhile, some comments regarding the structure of the regression and the measurement of the dependent variable are appropriate at this point.

First, note that the coefficients b_1 , b_2 , and b_3 in (1) have clear interpretations as conditional estimates of population cell mean differences. That is, after adjusting for any effects of firm size, the expected values of the coefficients are:

$$E(\hat{b}_1) = \mu_3 - \mu_1 ;$$

$$E(\hat{b}_2) = \mu_2 - \mu_1 ; \text{ and}$$

$$E(\hat{b}_3) = (\mu_4 - \mu_3) - (\mu_2 - \mu_1).$$

Thus, $H_{a1} - H_{a3}$ may be tested by assessing the algebraic sign and statistical significance of $\hat{b}_1$, $\hat{b}_2$, and $\hat{b}_3$, respectively.

Second, note that $\hat{u}_{jt}$, a measure of nonsystematic return, is being substituted as a dependent variable for the R_{jt} of Chapter 4. In that chapter, emphasis was placed on obtaining unbiased estimates of average lessee "abnormal performance" during the various event period intervals, and portfolio methods were relied on to free these estimates of biases due to extreme measurement error in the β_j of individual firms. Since, in the present context, regression (1) is to

be estimated at the individual firm level, use of a "full return" specification of the model, i.e.,

$$R_{jt} = c_0 + c_1 \hat{\beta}_{jt} + \dots$$

would simply have the effect of reintroducing these biases. It should, however, be possible to construct reasonably unbiased measures of the alternate dependent variable u_{jt} by making use of the portfolio-based coefficient estimates $\hat{a}_1$ and $\hat{a}_2$ developed in Chapter 4. Thus,

$$\hat{u}_{jt} = R_{jt} - \hat{a}_{1t} - \hat{a}_{2t} \hat{\beta}_{jt}, \quad (2)$$

where

$\hat{\beta}_{jt}$ = the local estimate of $\hat{\beta}_j$ corresponding to period t ;

$\hat{a}_{1t}, \hat{a}_{2t}$ = estimated coefficients from regression II (described in Chapter 4 and based on 10-security portfolios) for period t ; and

R_{jt} = the realized return for equity j in period t .

As mentioned earlier, (1) will also be estimated in "aggregate" form, and in this case the dependent variable will be defined as

$$\hat{u}_j = \frac{1}{82} \sum_t n_t \hat{u}_{jt}, \quad (3)$$

where n_t is the number of weeks in the t^{th} event period interval.

Thus, $\hat{u}_j$ measures the average per-week nonsystematic return component for firm j over the 82 weeks comprised by the 11 event period intervals.

Finally, notice that, in addition to variables representing potential financial statement impact and leverage, a firm size variable has also been included in (1). While the latter is not particularly relevant in terms of the Chapter 3 discussion of contracting effects, there are nevertheless good reasons for its inclusion here. It is well established empirically (e.g., Banz (1981), Reinganum (1981), Keim (1983)) that the nonsystematic component of security returns tends to vary with size. The direction of covariation shifts from time to time, and its cause is not well understood, but its apparent influence is difficult to overlook. One plausible explanation for the size effect is simply that firm size proxies strongly for the firm's sensitivity to an extra-market common factor which influences returns generally. This explanation, which makes something of a mockery of the typical empirical definition of nonsystematic return, has been tested by Reinganum (1982) with somewhat negative results. On the other hand, data presented in a recent paper by Jain (1982) can be interpreted as strongly supporting a "missing factor"-type explanation for the size effect. Firm size was ignored in the cross-sectional regressions described in Chapter 4 because the size distributions of lessee and non-lessee firms were not very different. In (1), however, the independent variables L_j , D_j , and $L_j D_j$ partition the total sample into four cells, and if particular ranges of the size spectrum tend to cluster in different cells, then inclusion of a size variable will have the desirable effect of eliminating any intercell return differences attributable to firm size alone.

Regression (1) is used as the basis for testing $H_{a1} - H_{a3}$. Suppose that the Chapter 4 results indicate significant negative abnormal performance for lessee firms generally in a given event period interval. If the contracting arguments of Chapter 3 provide an explanation for such abnormal performance, then one would expect $\hat{b}_3$ to be significantly negative for that interval. Coefficients $\hat{b}_1$ and $\hat{b}_2$ should be either negative or indistinguishable from zero, depending on the degree of detectability of smaller order contracting effects. Positive coefficients would be expected for those event period intervals in which lessee abnormal performance was significantly positive. Finally, since the Chapter 4 results suggest that lessee return performance was abnormally poor over the combined event period intervals, one would expect negative coefficients when (1) is estimated in aggregate form.

In order to provide evidence concerning H_{a4} and H_{a5} , an augmented form of (1) was also estimated:

$$\begin{aligned}\hat{u}_{jt} = & b_0 + b_1L_j + b_2D_j + b_3L_jD_j + b_4LSIZE_j \\ & + b_5\lambda'_j + b_6\lambda'_jZ_{j2} + b_7\lambda'_jZ_{j3} + b_8\lambda'_jZ_{j4} \\ & + b_9P_j + b_{10}P_jZ_{j2} + b_{11}P_jZ_{j3} + b_{12}P_jZ_{j4} + v_{jt},\end{aligned}\quad (4)$$

where $\hat{u}_{jt}$, L_j , D_j , and $LSIZE_j$ are as defined previously, and

λ'_j = proportion of firm j 's long-term debt which is subject to accounting-based restrictions;

P_j = proportion of long-term debt subject to accounting restrictions which is publicly traded;

$$Z_{j2} = \begin{cases} 1 & \text{if } L_j = 0 \text{ and } D_j = 1, \text{ and} \\ 0 & \text{otherwise;} \end{cases}$$

$$Z_{j3} = \begin{cases} 1 & \text{if } L_j = 1 \text{ and } D_j = 0, \text{ and} \\ 0 & \text{otherwise;} \end{cases}$$

$$Z_{j4} = \begin{cases} 1 & \text{if } L_j = 1 \text{ and } D_j = 1, \text{ and} \\ 0 & \text{otherwise.} \end{cases}$$

Basically, (4) allows for variation in $\hat{u}_{jt}$ due to variation in the proportion of debt subject to accounting-based restrictions (λ'_j) and in the proportion of such debt which is publicly traded (P_j).

Inclusion of the dummy variables Z_{jk} should make it possible to detect differences across cells in the sensitivity of $\hat{u}_{jt}$ to these debt characteristics.

Except for efficiency considerations, estimating (4) is equivalent to estimating separate regressions of the form

$$\hat{u}_{jt} = \gamma_0 + \gamma_1 \text{LSIZE}_j + \gamma_2 \lambda'_j + \gamma_3 P_j + z_{jt}$$

for each of the cells represented in Table 5-1. In purely econometric terms, (4) can be viewed as an estimating equation which permits both intercept (γ_0) and slope coefficients for λ' and P (γ_2 and γ_3) to vary uniquely from cell to cell, as indicated below:

<u>Cell</u>	<u>γ_0</u>	<u>γ_2</u>	<u>γ_3</u>
1	b_0	b_5	b_9
2	$b_0 + b_2$	$b_5 + b_6$	$b_9 + b_{10}$
3	$b_0 + b_1$	$b_5 + b_7$	$b_9 + b_{11}$
4	$b_0 + b_1 + b_2 + b_3$	$b_5 + b_8$	$b_9 + b_{12}$

The economic interpretation of coefficients $\hat{b}_1$, $\hat{b}_2$, and $\hat{b}_3$ also shifts as one moves from regression (1) to regression (4). In the former instance these coefficients can, as pointed out earlier, be interpreted as estimates of various cell mean differences when firm size is held constant. Thus, the hypothesized financial statement impact x leverage interaction should be manifested as a significant value for $\hat{b}_3$. In regression (4), however, $\hat{b}_1$, $\hat{b}_2$, and $\hat{b}_3$ must be interpreted as cell mean differences when (a) firm size is held constant, and (b) the proportion of debt subject to accounting restrictions is zero. In this case, one would expect $b_1 = b_2 = b_3 = 0$, and any financial statement impact x leverage interaction should show up in the form of differences across cells in the slope coefficients of λ' and/or P. In particular, one would expect the $\hat{u}_{jt}$ of firms falling in Cell 4 to be most sensitive, and the $\hat{u}_{jt}$ of firms falling in Cell 1 to be least sensitive, to the levels of λ' and P.

5.4 Description of Sample and Data Collection Procedures

With some adjustments for missing data, the sample of firms used for the present analysis is identical to the sample described in Chapter 4. For each of the 392 firms in that sample, an attempt was made to collect the following basic set of data:

1. total accounting assets (TA).
2. book value of long-term debt (LTD).
3. present value of noncapitalized financing leases, per ASF 147 (NCL).
4. market value of common equity (MVE).

5. various characteristics of individual issues or classes of long-term debt:
 - a. public v. private.
 - b. secured v. unsecured.
 - c. maturity.
 - d. callable, convertible, etc.
 - e. rating, if publicly traded.
 - f. interest rate:
 - i. level.
 - ii. fixed v. floating.
 - g. subject or not subject to accounting-based loan restrictions.
 - h. nature of accounting restrictions, if any.

The first four items were taken directly from the Standard and Poors Compustat file. TA, LTD, and NCL are the figures reported for the fiscal year end at which a given firm first complied with the ASR 147 disclosure requirements, falling generally during the one year period centered on December 31, 1973. MVE was determined based on the closing stock price for December 31, 1973.

The various data listed under item 5 above were collected primarily from the 1973 and 1974 volumes of Moodys Industrial Manual, and in some instances from 10K reports submitted to the SEC for the first ASR 147 compliance year. Moodys is a convenient source of two distinct classes of information concerning the long-term debt of most firms whose common stock is publicly traded. First, a detailed account of each outstanding publicly traded bond issue is provided. This account includes the bond's rating, the exchange on which it is listed, the trustee for the issue, and a reasonably detailed summary of the terms of the indenture itself. Second, Moodys also includes a more or less complete reproduction of each firm's annual report/10K footnote disclosures concerning long-term debt. The latter class of

information, it should be pointed out, is far less detailed than the former.

The collection of items 5a - 5h for public issues, and (at least an approximation of) items 5a - 5f for private issues, was time consuming but fairly straightforward. On the other hand, it must be admitted that for private issues the quantification of items 5g and 5h -- i.e., information concerning accounting-based loan restrictions -- is far less than perfect. While firms are required under generally accepted accounting principles to disclose the existence of significant accounting restrictions, the disclosures are frequently so lacking in detail that it is impossible to relate specific restrictions to specific loans. Because the proportion of long-term debt subject to accounting restrictions has been specified as an independent variable in regression (4), this situation raises a policy question: should the firm in question be deleted from the sample on the grounds that available information is insufficient to quantify this variable, or should an "educated guess" be made as to the relationship between accounting restrictions and specific private loans?

It was decided to opt for the second alternative for two reasons. First, it became apparent, based on a review of Moodys data for all 392 firms, that if the first alternative were followed, as many as half of the firms in the total sample could be lost. Second, and based on the same review, it became equally apparent that certain types of private loans -- unsecured promissory notes payable to institutional lenders and term loan/revolving credit arrangements with

commercial banks -- are predictably subject to accounting restrictions, while other types -- mortgage bonds and notes, foreign debt generally, and capitalized lease obligations -- are typically not subject to such restrictions. Thus, in cases where it was clear that at least some of a firm's private loans were subject to accounting restrictions, but not which loans, it was assumed that those falling into the former category were so subject, and that those in the latter category were not.

In arriving at the final sample to be used in the present analysis, firms were dropped for two reasons. First, if the Moodys data were insufficiently detailed to allow at least a crude determination of the dollar amounts of public and private debt subject to accounting restrictions, the relevant 10K filing was checked. If the 10K disclosures concerning long-term debt were equally insufficient, or if the 10K filing was not locally available, the firm in question was deleted. Forty two firms were lost for this reason. Second, a firm was deleted if returns data were unavailable for any one of the eleven event period intervals defined in Chapter 4. Ten additional firms were lost for this reason. The final sample, therefore, consists of 340 firms, of which 166 are lessees and 174 are non-lessees.

5.5 Measurement of Independent Variables

This section provides a brief description of the basis on which categorical values are assigned to the regression variables L_j and D_j , and of the measures used to represent λ'_j , P_j , and $LSIZE_j$.

The variable L_j is meant to capture the potential financial statement impact of an accounting standard which imposes incrementally stringent lease capitalization criteria. If NCL_j (as defined in Section 5.4), which represents the dollar amount of j 's potentially capitalizable leases, is large when considered in relation to the scale of the firm's financial statements, one would expect both the present and future potential impact of such a standard to be large, and vice versa. Sample firms are therefore ranked according to the ratio NCL_j/TA_j ; L_j is then assigned a value of 1 if this ratio is "large," and 0 otherwise. Given the procedures used to select the sample, however, there already exists a natural division between "large" and "small" lessees. Recall that, for firms in the non-lessee subsample, NCL_j/TA_j is less than 0.01, while for firms in the lessee subsample, NCL_j/TA_j exceeds 0.05. Thus, when regressions (1) and (4) are estimated for the total sample, L_j is assigned a value of 1 if firm j is a lessee firm, and a value of 0 if firm j is a non-lessee firm.

In order to capture the leverage component (C/S) of equation (7) in Chapter 3, sample firms are ranked according to the ratio $(LTD_j + NCL_j)/MVE_j$. The dichotomous variable D_j is then assigned a value of 1 (0) if this ratio falls above (below) the sample median value. Notice that the ranking ratio is simply a debt-equity ratio, and that NCL_j , the present value of noncapitalized financing leases per ASR 147, is included in the numerator in order to provide some degree of commensurability across lessee and non-lessee firms.

The size variable, $LSIZE_j$, is measured as the natural logarithm of the sum of: book value of total debt, market value of common equity, and the present value of noncapitalized financing leases per ASR 147. The latter component is again included to provide comparability across lessee and non-lessee firms, and the log form is used because the sum in question is highly skewed.

Finally, λ'_j and P_j are measured in a straightforward manner. The first of these, λ'_j , is simply the ratio of book value of long-term debt subject to accounting-based restrictions to the sum of book value of long-term debt and present value of noncapitalized financing leases. The second, P_j , is the ratio of public debt subject to accounting-based restrictions to total long-term debt subject to accounting-based restrictions.

These measures are summarized in Table 5-2.

5.6 Results

Tables 5-3 and 5-4 provide statistics which summarize the marginal and joint distributions of NCL_j/TA_j , $(LTD_j + NCL_j)/MVE_j$, $LSIZE_j$, λ'_j , and P_j , the variables discussed in the preceding section. Also included in Table 5-3 are similar statistics for two additional measures not previously defined: LTD_j/MVE_j and λ'^*_j . The first of these is simply the more conventional form of the debt-equity ratio; the second, λ'^*_j , is the proportion of "booked" long-term debt which is subject to accounting-based restrictions. These data suggest that the lessee and non-lessee subsamples are strikingly different as regards degree of leverage, with the former in general being

Table 5-2

Independent Variable Definitions

$$L_j = \begin{cases} 1 & \text{if firm } j \text{ is a "lessee" firm, and} \\ 0 & \text{otherwise;} \end{cases}$$

$$D_j = \begin{cases} 1 & \text{if } (LTD_j + NCL)/MVE_j \text{ falls above sample median, and} \\ 0 & \text{otherwise;} \end{cases}$$

$$\lambda_j' = \frac{LTDAR_j}{LTD_j + NCL_j}$$

$$P_j = \frac{PUB_j}{LTDAR_j} ; \text{ and}$$

$$LSIZE_j = \ln(TA_j - EVE_j + MVE_j + NCL_j).$$

EVE_j = book value of common equity.

LTD_j = book value of long-term debt.

$LTDAR_j$ = book value of long-term debt subject to accounting-based restrictions.

MVE_j = market value of common equity.

NCL_j = present value of noncapitalized financing leases, disclosed per ASR 147.

PUB_j = book value of public debt subject to accounting-based restrictions.

TA_j = total accounting assets.

Accounting variables are as reported for fiscal year of first disclosures under ASR 147, generally at or near 12/31/73. MVE_j is based on 12/31/73 closing stock price.

Table 5-3

Regression Variables
- Descriptive Statistics -

Variable		Total Sample	Lessee Subsample	Non-lessee Subsample
NCL/TA	Average	0.117	0.240	-
	Median	0.000	0.149	-
	Q3 - Q1 ^a	0.144	0.234	-
(LTD+NCL)/MVE	Average	1.606	2.666	0.594
	Median	0.741	1.703	0.245
	Q3 - Q1	1.702	2.685	0.710
LTD/MVE	Average	1.036	1.500	0.594
	Median	0.548	0.915	0.245
	Q3 - Q1	1.050	1.486	0.710
LSIZE	Average	5.242	5.359	5.130
	Median	5.201	5.237	5.131
	Q3 - Q1	2.672	2.410	3.020
λ'	Average	0.375	0.331	0.417
	Median	0.373	0.332	0.407
	Q3 - Q1	0.653	0.428	0.835
λ'^*	Average	0.500	0.586	0.417
	Median	0.534	0.680	0.407
	Q3 - Q1	0.896	0.700	0.835
P	Average	0.176	0.185	0.167
	Median	0.000	0.000	0.000
	Q3 - Q1	0.199	0.291	0.000
Sample Size		340	166	174

^a Interquartile range.

Table 5-4
 Regression Variables
 - Product Moment Correlations -

	<u>NCL</u> <u>TA</u>	<u>LTD + NCL</u> <u>MVE</u>	Lsize	λ'	P
<u>NCL</u> <u>TA</u>	1.00	0.43	0.03	-0.23	-0.00
<u>LTD + NCL</u> <u>MVE</u>		1.00	-0.02	0.02	0.10
Lsize			1.00	-0.10	0.27
λ'				1.00	0.21
P					1.00

substantially more highly leveraged than the latter. In addition, the statistics for λ^* indicate that accounting-based loan covenants are more abundantly present in the recorded debt of lessees than in that of non-lessees. This pattern is consistent with (but does not imply the validity of) the frequent claim that "off-balance-sheet" financing is an important motivation for leasing. When debt is defined to include the present value of non-capitalized financing leases, it appears that the proportion of debt subject to accounting restrictions is actually slightly smaller for lessees than for non-lessees, as evidenced by the statistics for λ' . Finally, another frequent contention -- that leasing is relatively preferred by small firms because the fixed costs associated with leasing are lower than those associated with other financing vehicles -- is not supported by the data. The size distributions of lessee and non-lessee firms are similar, and the product-moment correlation between LSIZE and NCL/TA is insignificant.

Estimation results for regressions (1) and (4) are shown in Table 5-5. The table includes coefficient estimates for each event period interval and also for the "aggregate" regressions defined in Section 5.3. In the case of individual event periods, numerical values are reported for all coefficients having "large" t-statistics ($|t| > 1.20$). Where $|t| < 1.20$, the estimated coefficient is indistinguishable from zero at conventional significance levels, and only the sign of the estimate is shown. This practice, which is adopted to make the table more easily readable, is suspended in the

Table 5-5 (continued)

Event Period		Estimated Coefficient of														R^2	R^2	R^2
Interval	Inter- cept	L	0	LD	LSIZE	λ^1	P	$\lambda^1 Z_2$	$\lambda^1 Z_3$	$\lambda^1 Z_4$	P^2_2	P^2_3	P^2_4	R^2	R^2			
5 (1)	0.0033 (1.36)	-	-	-	-0.0006 (-1.50)	x	x	x	x	x	x	x	x	x	0.017	0.006		
(4)	+	+	+	-	-0.0006 (-1.46)	0.0065 (2.17)	-	-0.0097 (-1.37)	-	-0.0077 (-1.35)	0.0101 (1.48)	+	+	+	0.036	0.001		
6 (1) ^a	-0.0151 (-4.13)	+	-	-0.0098 (-1.92)	0.0033 (5.37)	x	x	x	x	x	x	x	x	x	0.132	0.122		
(4) ^a	-0.0138 (-3.37)	-	-	-	0.0031 (4.77)	-	+	+	+	-	-	+	+	+	0.144	0.112		
7 (1)	-0.0048 (-1.84)	+	+	-	0.0009 (2.06)	x	x	x	x	x	x	x	x	x	0.017	0.006		
(4)	-0.0043 (-1.45)	+	+	-	0.0008 (1.69)	-	+	-	+	-	+	-	-	-	0.024	-0.012		
8 (1) ^a	-0.0103 (-3.73)	+	+	0.0076 (2.00)	0.0020 (4.46)	x	x	x	x	x	x	x	x	x	0.109	0.098		
(4) ^a	-0.0115 (-3.80)	-	-	0.0184 (2.67)	0.0021 (4.31)	+	-	+	+	-0.0138 (-2.19)	+	+	-	-	0.128	0.096		
9 (1) ^a	-0.0086 (-3.55)	-0.0049 (-1.87)	+	+	0.0016 (3.74)	x	x	x	x	x	x	x	x	x	0.058	0.046		
(4) ^a	-0.0096 (-3.37)	-	+	-	0.0016 (3.55)	+	-	-	+	+	-	-0.0156 (-1.71)	+	+	0.074	0.040		

Table 5-5 (continued)

Event Period Interval	Inter- cept	Estimated Coefficient of													R^2
		L	D	LD	LSIZE	λ^1	P	$\lambda^1 \lambda^2_2$	$\lambda^1 \lambda^2_3$	$\lambda^1 \lambda^2_4$	P^2_2	P^2_3	P^2_4	R^2	
10	(1)	-	0.0037 (1.61)	-	+	x	x	x	x	x	x	x	x	x	0.003
	(4)	-	+	-	+	+	-	-	-	+	+	+	-	-	0.018
11	(1)	-	0.0041 (1.61)	-	-	x	x	x	x	x	x	x	x	x	0.010
	(4)	-	+	-	+	-	0.0062 (1.34)	+	+	-0.0136 (-1.83)	-0.0118 (-1.24)	-0.0117 (-1.95)	-0.006	-0.006	-0.006
Aggregate															
	(1)	-0.0052 (-5.39)	0.0001 (0.13)	0.0007 (0.77)	-0.0029 (-2.19)	0.0010 (6.22)	x	x	x	x	x	x	x	x	0.151
	(4)	-0.0056 (-5.23)	0.0001 (0.10)	0.0006 (0.31)	-0.0017 (-0.69)	0.0010 (5.89)	0.0012 (1.05)	-0.0008 (-0.40)	0.0045 (0.41)	-0.0036 (-1.60)	0.0010 (0.37)	-0.0023 (-0.67)	0.0011 (0.51)	0.162	0.131

case of the aggregate regressions, for which all coefficient estimates and associated t-statistics are shown.

Consider first the aggregate regression results, shown in the final panel of Table 5-5. The aggregate version of (1) suggests that, when the 82 weeks encompassed by the 11 event period intervals are taken as a whole:

1. There is a strong relationship between non-systematic return and firm size, with small firms earning significant negative abnormal returns. Since LSIZE is the log of a size measurement taken in million dollar units, the regression intercept can be interpreted as the average weekly abnormal return for a non-lessee firm with below-median leverage whose size is \$1 million. The extreme negative abnormal performance indicated for firms in this category -- about -0.5% per week -- diminishes by about one tenth of one percent for each unit increase in LSIZE. Thus, the average weekly abnormal performance for low-leverage non-lessees of average LSIZE (LSIZE = 5.24) is approximately zero. It should be pointed out that this positive relationship between firm size and abnormal return is consistent with the findings of other studies which examine the same time period (e.g., Keim (1983)).¹
2. For a given level of firm size, the abnormal returns of non-lessees with above-median leverage are statistically indistinguishable from those of non-lessees with below-median leverage, as evidenced by the estimated coefficient of D.

3. For a given level of firm size, the abnormal returns to lessees with below-median leverage are statistically indistinguishable from those to non-lessees with below-median leverage, as evidenced by the estimated coefficient of L.
4. The hypothesized financial statement impact x leverage interaction is clearly present, as evidenced by the significant negative coefficient estimated for LD. In fact, the estimated coefficients of L, D, and LD can be given the following joint interpretation: while abnormal returns to lessees and non-lessees are indistinguishable when leverage is relatively small, highly leveraged lessees tend to earn substantially more negative abnormal returns than highly leveraged non-lessees.

Moving from the aggregate version of (1) to the aggregate version of (4), the following additional observations can be made:

1. Considering both degree of significance and order of magnitude, the intercept and the coefficients of L, D, and LSIZE appear to be stable across the two regressions.
2. In (1), it will be remembered, $\hat{b}_3$ is an estimate of

$$(\mu_4 - \mu_3) - (\mu_2 - \mu_1),$$

where μ_k is the mean abnormal return for firms falling in cell k of Table 5-1. In (4), however, $\hat{b}_3$ estimates

$$(\mu_4 - \mu_3) - (\mu_2 - \mu_1) | (\lambda' = 0 \text{ and } P = 0).$$

That is, in (4), $\hat{b}_3$ is a conditional estimate for an hypothetical group of firms whose loan contracts are unrestricted in accounting terms. Under the contracting

hypothesis, as was explained in Section 5.3, one would therefore expect $\hat{b}_3$ to be significant in (1) but indistinguishable from zero in (4). As can be seen, the aggregate regression results conform to this expectation.

3. The coefficients of λ' , $\lambda'Z_2$, and $\lambda'Z_3$ are statistically indistinguishable from zero, indicating that for both low- and high-leverage non-lessees, and for low-leverage lessees, there is no detectable relationship between $\hat{u}_j$ and the proportion of long-term debt subject to accounting restrictions. The coefficient of $\lambda'Z_4$, on the other hand, is both negative and significant (10% level, one-tailed test), suggesting that in the case of high-leverage lessees, $\hat{u}_j$ varies inversely with the proportion of long-term debt subject to accounting restrictions.
4. The coefficients associated with the "public debt" variables are everywhere indistinguishable from zero.
5. Finally, it is of interest to note, in comparing the aggregate versions of (1) and (4), that the addition of the various λ' and P variables does not actually improve the explanatory power of the regression. The incremental R^2 of about 0.01 is clearly insignificant. The primary difference between the two regressions seems to be a simple transfer of statistical significance from the coefficient of LD to the coefficient of $\lambda'Z_4$.²

On the whole, the aggregate regression results just described conform to the general pattern which would be expected under the

contracting hypothesis. It appears that on average, over the 82 weeks encompassed by the 11 event period intervals, high-leverage lessees earned significant negative abnormal returns relative to both low- and high-leverage non-lessees and to low-leverage lessees. Moreover, within the group of high-leverage lessees, the magnitude of abnormal performance appears to have been directly related to the proportion of debt subject to accounting-based loan restrictions.

With respect to the specific alternative hypotheses presented in Section 5.2, it can be said that the aggregate regression results support H_{a3} and partially support H_{a4} , but fail to support H_{a1} , H_{a2} , and H_{a5} . In the case of both H_{a1} and H_{a2} , this failure is not particularly bothersome. While the contracting hypothesis suggests that firms falling in cells 2 and 3 should exhibit negative abnormal performance relative to firms falling in cell 1, it is readily conceivable that such abnormal performance could be quite small in magnitude, and therefore, given the extreme "noisiness" of security returns, unlikely to be detected. That the regression results do not support H_{a5} is more interesting: on its face, this finding suggests that the public/private distinction is, contrary to the claims of some private lenders (and also to hypotheses based on the differential direct costs involved in renegotiating public and private loans), irrelevant insofar as contracting share price effects are concerned. It should be pointed out, however, that P is rather small for the average firm in the sample. Moreover, for the sample as a whole, there is a significant positive correlation (0.21) between P and λ' . Thus, it is possible that a more sophisticated level of analysis could

unearth a public/private effect which is masked by the present research design.

Unlike the aggregate results, which conform rather well to the contracting hypothesis, the regression results for the individual event period intervals can best be described as chaotic. In fact, if both algebraic sign and statistical significance are taken into account, the pattern of coefficients observed in connection with the aggregate regressions is not precisely replicated in even one of the eleven event period intervals. It can also be seen that, with the exception of periods 6 and 8, the explanatory power of the individual event period regressions is rather low in comparison to that of the aggregate regressions. One interpretation of this state of affairs is that the aggregate results are supportive of the contracting hypothesis only by coincidence. A more generous interpretation, and one which the author feels to be more reasonable, is simply that aggregation of the $\hat{u}_{jt}$ has a noise-reducing effect. The "true" u_{jt} are clearly noisy to begin with, in that they include a great deal of randomness which is unrelated to the accounting events of interest. And the measurement process itself adds considerable additional noise, in that the measured values of u_{jt} depend on highly imperfect β -estimates for individual firms. When the $\hat{u}_{jt}$ are aggregated across the 11 event period intervals, the effect is that of adding independent errors, and both sources of noise should be diminished. With regard to β -estimate-induced noise in particular, it should be recalled that the $\hat{u}_{jt}$ for each firm are calculated using seven independent and very localized β -estimates.

An implication of the "noise reduction" explanation for the marked difference between aggregate and individual event period regression results is that no attempt should be made to provide a detailed ex post rationalization for each of the latter, and no such attempt will in fact be made. However, it should be pointed out in passing that there are a few gross regularities in the event period regressions. First, the "size effect" is frequent and sometimes quite strong in terms of statistical significance. Second, the financial statement impact $\times$ leverage interaction, represented by the coefficient of LD, also appears at a significant level with some regularity (in five of the eleven event periods, to be precise). Lastly, the coefficient of $\lambda'Z_4$ is significant with marginally greater frequency than those of λ' , $\lambda'Z_2$, or $\lambda'Z_3$.

A second implication of the noise reduction explanation is that if one wishes to examine the sensitivity of the regression results to alternate specifications, such an examination should be undertaken with respect to aggregate results only. This question is addressed in the next section.

5.7 Robustness of Results

In this section, the results reported in Table 5-5 are compared with those obtained under various alternative specifications for regressions (1) and/or (4). The first such alternative involves a redefinition of the dependent variable. The $\hat{u}_{jt}$ are aggregated not over all eleven event period intervals, but only over those for which it was concluded (in Chapter 4) that lessee abnormal performance was

significant. The latter specification, which will be referred to as $\hat{u}_j'$, is similar to the dependent variable used in the Leftwich study. Regressions (1) and (4) are re-estimated on this basis, and results are reported in Table 5-6.

In the second alternative specification, the numerator of λ' is redefined to exclude from "long-term debt subject to accounting-based restrictions" all floating rate loans and any loans maturing prior to 1978. The independent variable P is redefined similarly. The redefined variables are substituted for λ' and P , regression (4) is re-estimated, and results are reported in Table 5-7. It must be admitted that the reason for this substitution is simply a twofold intuition: first, that the wealth transfer potential associated with loans of relatively short maturity is small, and second that creditor cooperation in solving loan covenant problems is likely to be achieved most costlessly when floating rate loans are involved. The primary effect of the substitution is to eliminate a large proportion of commercial bank term loans from the numerator of λ' . Rates on such loans are almost invariably geared to the prime rate, and maturities are frequently five years or less.

In the third alternative specification, D is redefined in terms of the sample median value of LTD/MVE , as opposed to the median of $(LTD + NCL)/MVE$. All other variables remain as defined in Table 5-2. This alternative is examined merely to see the pattern of coefficients which occurs when a more conventional definition of leverage is employed as a partitioning variable. Regressions (1) and (4) are re-estimated on this basis, and results are shown in Table 5-8.

As explained earlier, the size variable is included in regressions (1) and (4) in order to guard against the possibility that $\hat{b}_1$, $\hat{b}_2$, and $\hat{b}_3$ will merely reflect intercell differences in mean firm size. This procedure is followed not for any strong theoretical reason, but simply because the "size effect" in security returns is a well-documented empirical regularity. It would also be well to inquire whether the regression results are influenced by intercell differences in industry membership, the latter being commonly recognized as a second source of empirical regularity in returns. If a diverse selection of industries is included in each of the cells of Table 5-1, and if no one industry is represented in undue proportion in any given cell, then "industry effects" might be thought to be of no particular importance in the analysis. As Table 5-9 suggests, the first of these conditions is met reasonably well by the sample. However, a cell-by-cell examination of industry composition indicates that two industries -- retailers and air transport -- are almost completely concentrated within the lessee subsample. The proportions of retail firms to total firms in cells 1-4 are 1%, 0%, 33%, and 36%, respectively. For air transport firms, the proportions are 0%, 0%, 5%, and 9%. Actually, because these industries are distributed more or less evenly across both of the lessee cells, one would not expect "retailness" or "air transportness" to have much of an influence on the pattern of the aggregate coefficients. For good measure, however, regressions (1) and (4) are re-estimated, with all variables as defined in Table 5-2, but including additional dummy variables to

represent membership in the retail and air transport industries.

Results are reported in Table 5-10.

In principle, the aggregate results shown in Table 5-5 should be replicable on any subsample of the total sample for which there is a reasonable degree of "spread" in the distributions of the partitioning variables. In particular, one would expect to see a pattern of coefficients corresponding to that of Table 5-5 if regressions (1) and (4) are estimated using the lessee subsample only, with partitioning based on the subsample median values of NCL/TA and $(LTD + NCL)/MVE$. Results for this final variation are reported in Table 5-11.

The effects of the various modifications just described can, for the most part, be described very briefly. Neither the substitution of $\hat{u}_j^1$ for $\hat{u}_j$, the alternative definitions of λ^1 and D , nor the inclusion of industry dummy variables seems to have a dramatic impact on the general tenor of the Table 5-5 results. There are differences, to be sure: a careful examination will reveal, for instance, that a t -statistic which was initially insignificant has become marginally significant in the case of a particular alternative specification. But on the whole, the pattern of the coefficients presented in Table 5-5 persists in Tables 5-6, 5-7, 5-8, and 5-10, as does the pattern of transition from regressions of form (1) to those of form (4).

In the "lessee only" regression results presented in Table 5-11, however, a sharp contrast is evident. In regression (1), the significant negative coefficient for D , together with the absence of a significant interaction effect, indicates that within the lessee

Table 5-6
Alternative Specification^a of Dependent Variable

(1) $\hat{u}_j^t =$		-0.0056 (-4.81)	-0.0005L _j (-0.46)	-0.0002D _j (-0.20)	-0.0029L _j ^D (-1.78)	+0.0011LSIZE _j ^F (5.71)	
		$R^2 = 0.161$		$\bar{R}^2 = 0.151$		$F = 16.01$	
(4) $\hat{u}_j^t =$		-0.0061 (-4.69)	-0.0007L _j (-0.45)	-0.0008D _j (-0.32)	-0.0016L _j ^D (-0.55)	+0.0012LSIZE _j ^F (5.46)	+0.0017λ _j ^t (1.19)
							-0.0019P _j (-0.91)
			-0.0002λ _j ^t Z _j ² (-0.05)	+0.0020λ _j ^t Z _j ³ (0.43)	-0.0030λ _j ^t Z _j ⁴ (-1.12)		
			+0.0017P _j ^Z _j ² (0.53)	-0.0011P _j ^Z _j ³ (-0.26)	+0.0033P _j ^Z _j ⁴ (1.23)		

$$R^2 = 0.171 \quad \bar{R}^2 = 0.140 \quad F = 5.61$$

^a $u_j^t = \frac{1}{53} \sum_t n_t \hat{u}_{jt}^t$, $t = 1, 3, 4, 5, 6, 8, 9$. All other variables are as defined in Table 5-2.

Table 5-7

Alternative Specification^a of λ'

$\hat{u}_j = -0.0056$ (-5.49)	$+0.0005L_j$ (0.39)	$+0.0008D_j$ (0.59)	$-0.0028L_j^D$ (-1.45)	$+0.0010LSIZE_j$ (6.04)	$+0.0023\lambda'_{FLj}$ (1.47)	$-0.0019P_{FLj}$ (-0.97)
$-0.0016\lambda'_{FLj}$ (-0.57)	$+0.0000\lambda'_{FLj}$ (-0.00)	$-0.0047\lambda'_{FLj}$ (-1.79)	$+0.0031P_{FLj}$ (1.33)			
$+0.0016P_{FLj}$ (0.54)	$-0.0022P_{FLj}$ (-0.62)	$+0.0031P_{FLj}$ (1.33)				

$$R^2 = 0.167 \quad \bar{R}^2 = 0.136 \quad F = 5.46$$

^a $\lambda'_{FLj} = \frac{\text{fixed rate debt subject to accounting restrictions, maturing 1978 or later}}{LTD_j + NCL_j}$

$P_{FLj} = \text{"public debt" fraction of } \lambda'_{FLj}$

All other variables are as defined in Table 5-2.

Table 5-8

Alternative Specification^a of D_j

$$(1) \hat{u}_j = -0.0055 \quad -0.0002L_j \quad +0.0011D_j \quad -0.0028L_j D_j \quad +0.0010LSIZE_j$$

$$(-5.76) \quad (-0.27) \quad (1.30) \quad (-2.33) \quad (6.49)$$

$$R^2 = 0.151 \quad \bar{R}^2 = 0.141 \quad F = 14.91$$

$$\hat{u}_j = -0.0059 \quad +0.0008L_j \quad +0.0018D_j \quad -0.0032L_j D_j \quad +0.0010LSIZE_j \quad +0.0015\lambda_j' \quad -0.0009P_j$$

$$(-5.53) \quad (0.19) \quad (1.14) \quad (-1.44) \quad (5.95) \quad (1.12) \quad (-0.45)$$

$$-0.0018\lambda_j' Z_{j2j} \quad -0.0028\lambda_j' Z_{j3j} \quad -0.0030\lambda_j' Z_{j4j}$$

$$(-0.74) \quad (-1.33) \quad (-1.48)$$

$$+0.0004P_j Z_{j2j} \quad +0.0002P_j Z_{j3j} \quad +0.0009P_j Z_{j4j}$$

$$(0.14) \quad (0.05) \quad (0.35)$$

$$R^2 = 0.159 \quad \bar{R}^2 = 0.129 \quad F = 5.16$$

$$a \quad D_j = \begin{cases} 1, & \text{if } \frac{LTD}{MVE_j} \text{ exceeds sample median, and} \\ 0 & \text{otherwise} \end{cases}$$

All other variables are as defined in Table 5-2.

Table 5-9
Industry Distribution
in
Total Sample Regressions

<u>Cell</u>	<u>Number of Unique SIC Codes</u>		
	<u>One Digit</u>	<u>Two Digit</u>	<u>Three Digit</u>
1	6	28	61
2	5	19	27
3	4	19	26
4	7	32	57

Table 5-10
Alternative Specification with Industry Dummy Variables

(1)	$\hat{u}_j =$	-0.0054 (-5.63)	+0.0008L _j (0.82)	+0.0007D _j (0.79)	-0.0027L _j D _j (-2.02)	+0.0019LSIZE _j (6.49)	-0.0019RTL _j (-2.26)	-0.0027AIR _j (-1.77)
		R ² = 0.168 R ² = 0.129 F = 6.72						
(4)	$\hat{u}_j =$	-0.0058 (-5.42)	+0.0011L _j (0.81)	+0.0006D _j (0.29)	-0.0007L _j D _j (-0.27)	+0.0011LSIZE _j (6.14)	-0.0029RTL _j (-2.99)	-0.0029AIR _j (-1.86)
		+0.0012λ _j -0.0010P _j (1.03) (-0.54)						
		-0.0005λ _j ¹ Z _j ¹ (-0.18)		+0.0016λ _j ¹ Z _j ¹ (0.42)		-0.0059λ _j ¹ Z _j ¹ (-2.47)		
		+0.0011P _j ² Z _j ² (0.37)		-0.0018P _j ² Z _j ² (-0.54)		+0.0019P _j ² Z _j ² (0.88)		
		R ² = 0.188 R ² = 0.153 F = 5.38						

Table 5-11

Regression Results Based on Lessee Subsample Only

$$(1) \hat{u}_j = -0.0054 \quad -0.00021_j \quad -0.0028D_j \quad +0.0017L_j D_j \quad +0.0011LSIZE_j \quad -0.0023RTL_j \quad -0.0028AIR_j \\ (-3.14) \quad (-0.19) \quad (-2.22) \quad (1.01) \quad (4.09) \quad (-2.23) \quad (-1.62)$$

$$R^2 = 0.173 \quad \bar{R}^2 = 0.141 \quad F = 5.52$$

$$(4) \hat{u}_j = -0.0010 \quad -0.0034L_j \quad -0.0008D_j \quad +0.0084L_j D_j \quad +0.0080LSIZE_j \quad -0.0022RTL_j \quad -0.0008AIR_j \\ (-0.41) \quad (-1.65) \quad (-3.02) \quad (2.60) \quad (2.80) \quad (-2.06) \quad (-0.42)$$

$$-0.0072\lambda_j' \quad +0.0018P_j \\ (-2.71) \quad (0.68)$$

$$+0.0086\lambda_j' Z_{2j} \quad +0.0062\lambda_j' Z_{3j} \quad +0.0035\lambda_j' Z_{4j} \\ (1.73) \quad (1.16) \quad (0.63)$$

$$+0.0062P_j Z_{2j} \quad -0.0011P_j Z_{3j} \quad -0.0075P_j Z_{4j} \\ (1.43) \quad (-0.32) \quad (-1.94)$$

$$R^2 = 0.264 \quad \bar{R}^2 = 0.195 \quad F = 3.86$$

sample itself, the most consistently negative price behavior is concentrated in cell 2 of Table 5-1. This result persists in regression (4), and is in fact magnified, since in the latter case the coefficient of LD is significantly positive. Moreover, the transition from (1) to (4) is marked by a kind of volatility that is absent in the earlier regressions. There are numerically large shifts in the estimated values for b_0 , b_1 , b_2 , and b_3 , and the estimated coefficients for the various λ' and P variables are frequently large and significant.

In order to more easily examine the relationships represented by the many coefficients involved, regression (4) is decomposed into four separate regressions, one for each cell of Table 5-1. Based on various assumptions regarding LSIZE, λ' , and P, an implied cumulative abnormal return can be calculated for firms falling in any given cell. A tabulation of such calculations is shown in Table 5-12 for an hypothetical non-retail, non-airline firm of average LSIZE. Each column of the table represents a different set of assumptions concerning λ' and P. Similar calculations based on the estimated coefficients of regression (1) are also presented.

Examining Table 5-12 on a column-by-column basis, it is clear that when considered in light of $H_{a1} - H_{a5}$, something is "wrong" with the results in every case. The regression (1) and regression (4): ($\lambda' = 1$, $P = 0$) columns are perhaps least disturbing. In these columns, the implied CAR's for cells 1 and 3 (2 and 4) are of the same order of magnitude, and it would be possible to devise a "story" that is consistent both with the contracting hypothesis and with these

Table 5-12

- Lessee-Only Regressions -
 Implied Cumulative
 Abnormal Returns^a
 (%)

<u>Cell</u>	Regression (1)	Regression (4)		
		$\lambda' = 0$ <u>P = 0</u>	$\lambda' = 1$ <u>P = 0</u>	$\lambda' = 1$ <u>P = 1</u>
1	2.98	26.17	-32.87	-18.11
2	-19.97	-39.43	-27.95	37.65
3	1.34	-1.71	-9.91	-4.17
4	-7.68	1.57	-28.78	-75.52

^a 82 week CAR for hypothetical non-retail,
 non-airline firm with LSIZE = 5.24.

results. The ($\lambda' = 0$, $P = 0$) and ($\lambda' = 1$, $P = 1$) columns for regression (4), on the other hand, do not appear to be susceptible of any explanation which is consistent with the contracting hypothesis. A glimmer of light arises in that the most negative implied CAR for the table as a whole occurs in ($\lambda' = 1$, $P = 1$), cell 4, but this isolated result should not be interpreted independently of others in the same column.

The disparity between the various total sample regression results and the lessee only regression results can be interpreted in three ways. First, it is possible that the contracting hypothesis simply lacks validity. In this case, the "consistent" results observed in the total sample regressions must be interpreted as coincidental or as an artifact of the research design. Second, it is possible that the general contracting hypothesis itself is valid, but that the empirical implications deduced in Chapter 3 are too simplistic to account for the full range of actual contracting effects. Finally, it is possible that both the general hypothesis and the Chapter 3 predictions are valid, but that in moving from the total sample regressions to the lessee only regressions, one observes the emergence of independent effects which in the total sample case were effectively "randomized away." Lacking an ability to distinguish among these possibilities, the author concludes that the results presented in this chapter, like those of the Leftwich study, are weakly supportive of the contracting hypothesis.

5.8 Notes

1. This result contradicts the "conventional wisdom" regarding the size effect -- namely, that small firms earn higher (i.e., more positive) risk-adjusted returns than do large firms. Two possible explanations may be offered in its defense. First, if u_{jt} is on average negative during period t (as is frequently the case in this study), and if $\text{var}(u_{jt})$ is generally greater for small firms than for large firms, then a positive relationship between u_{jt} and LSIZE can be expected. This notion has been developed in detail by Jain (1982). Second, recent work by Keim (1983) and by Brown, Kleidon and Marsh (1983) indicates that the direction of the size effect is simply not constant over time. In particular, large firms appear to have earned higher risk-adjusted returns than small firms over the 1969-1974 period.
2. As noted earlier in this chapter, the inclusion of L , D , and LD as independent variables in (4) in effect permits the regression intercept to vary uniquely across the cells of Table 5-1. The "aggregate" regression (4) estimates of b_1 , b_2 , and b_3 , however, fail to reject the hypothesis of identical intercepts across cells. When these variables are excluded from the regression -- that is, when the intercept term is constrained to be constant across cells -- the general pattern of the remaining coefficient estimates does not change significantly, and $\hat{b}_8$ remains significantly negative.

CHAPTER 6

SUMMARY AND CONCLUSIONS

The research described in this dissertation has been carried out with two objects in view. The first of these is to determine if the behavior of share prices during the 1973-1976 lease accounting standard-setting process was consistent with the predictions, voiced repeatedly by lessees and their representatives, that prices would suffer a downward revision in the event of a mandated increase in the level of lease capitalization. The second is to inquire whether apparent adverse price revisions (if any) conform in cross-section to a pattern predicted by an increasingly familiar hypothesis: that mandated changes in financial reporting practice may, by effectively altering accounting-based terms in existing loan contracts, impose more-than-trivial costs on the shareholders of affected firms.

The first and most general of these questions has been addressed in a number of previous studies, which were reviewed in detail in Chapter 2. While different authors draw different inferences from the data, the results of these studies, taken as a whole, suggest that abnormally negative share price behavior was associated with various policy process events during the period indicated. However, "association" in this context merely means "correlation," and no causal relationship between accounting policy events and price behavior is implied by correlation per se.

If one's interest is to increase the level of confidence with which it can be asserted that such an association is actually symptomatic of a causal relationship, it would appear that there are three possible routes to follow. The first of these is to seek evidence of the same behavior in a similar but independent context. There is at present, for instance, a limited set of evidence which suggests that two other mandated changes in financial reporting policy -- APB Opinion No. 16 and SFAS No. 19 -- have also been associated with negative share price behavior. With respect to the former, especially, the results of the event-related abnormal performance tests reported by Leftwich (1981) are particularly persuasive, in that they consider the totality of events comprised by the APB 16 policy process. This is in contrast to the SFAS 19 studies -- Collins and Dent (1979) and Lev (1979) -- which are based only on one or two "primary" events.

A second possibility is to identify "more than remotely possible" alternative explanations for the observed association, and to determine whether, after controlling for these, the association vanishes. This route was followed for at least some distance in the present study. Chapter 2 identifies two competing explanations for the association between lessee accounting events and share price behavior, each of which arises from a potential deficiency in procedures commonly used to measure the nonsystematic component of security returns. One of these, termed the "missing factor problem," results from an incomplete specification of the returns-generating process. The second, referred to as the "nonstationarity problem,"

results if errors in measuring the parameters of the returns-generating process are not randomly distributed across firms.

Chapter 4 describes tests which are designed to detect the presence of abnormal price behavior during brief intervals surrounding each of 16 lessee accounting-related events which occurred during the 1973-1976 policy change period. Explicit in these tests is the assumption, or maintained hypothesis, that the returns generating process is driven by a single common factor. That is, the missing factor problem mentioned above is assumed away for purposes of analysis, as was done in three of the previous lessee accounting studies reviewed in Chapter 2. Within the bounds of this assumption, however, a serious and painstaking effort is made to eliminate the nonstationarity problem.

The basic results of these abnormal performance tests are three-fold:

1. When attention is restricted to "event weeks" only, and after controlling for common variation in returns, lessee returns are found to differ significantly (on average) from those to non-lessees in 5 of the 16 weeks. When abnormal performance estimates are cumulated over the 16 weeks, however, the lessee/non-lessee difference is small and statistically indistinguishable from zero.
2. When attention is focused on "event period intervals," each consisting of one or two event weeks plus the three weeks preceding and the three weeks following, significant and generally negative lessee abnormal performance is noted in 7

of 11 such intervals. On a cumulative basis over the eleven event periods, and after controlling for common variation in returns, lessee equities appear to have declined in value, relative to non-lessees, by a statistically significant 13.5%.

3. When a similar analysis is conducted in the absence of the procedures adopted to control for the nonstationarity problem, the resulting cumulative (event period interval) estimate of lessee abnormal performance is also negative, but of substantially greater magnitude (about -23%).

These results are in general agreement -- although they differ in some particulars -- with those of three of the four studies reviewed in Chapter 2 (i.e., Ro (1978), May, Harkins and Rice (1978), and Pfeiffer (1980)). Direct comparison with the fourth study, Abdel-khalik, Ajinkya, and McKeown (1978) is relatively difficult. It is the author's opinion that the Chapter 4 results will support the following interpretation. First, the nonstationarity problem is a non-trivial problem, at least in the period under study, in that failure to control for differential drifts in lessee and non-lessee s has a magnifying effect on estimates of lessee abnormal performance. Even after controlling for this problem, however, the data continue to indicate that lessee equities experienced abnormal declines in value relative to non-lessee equities over the eleven event period intervals examined. In this sense, the data remain consistent with the hypothesis that the 1973-1976 policy process caused a downward revision in lessee share prices. Finally, "missing factor" caveats remain appropriate for all results reported in Chapter 4.

In attempting to assess whether an association between share price activity and policy process events is indicative of a causal relationship, the third route which may profitably be followed is to develop an explanation for the hypothesized relationship which has testable implications beyond mere abnormal share price behavior. One such explanation which has traditionally been offered by lobbying firms, accounting practitioners, and (more recently), accounting researchers, has been referred to here as the "contracting hypothesis." This hypothesis asserts that share price adjustments should be expected in connection with a financial reporting standard-setting process if shareholders are party to contracts which include accounting-based terms defined with reference to generally accepted accounting principles, and if it is probable that the rule change contemplated by the process will have a material impact on financial statement numbers and relationships.

Chapter 3 provides a detailed discussion of the contracting hypothesis as it relates to loan contracts, in which accounting numbers often play a conspicuous role. The adverse impact of expanded lease capitalization on accounting-based loan contract terms is described, and several testable implications for share price behavior are specified. Two of the latter have not, to the author's knowledge, been explicitly considered in any previous study. The first of these is that a financial statement impact $\times$ leverage interaction should be discernible in the cross-sectional distribution of abnormal returns. The second is that the magnitude of contract-related abnormal performance should increase as the proportion of debt subject to accounting-based restrictions increases. A third implication, first

proposed in the Leftwich (1980) study, is that the magnitude of contract-related abnormal performance should increase as the "public" proportion of such debt increases.

The cross-sectional regression estimates reported in Chapter 5 provide some support for these predictions. Tests based on a large sample composed of both lessee and non-lessee firms indicate that over the eleven event period intervals taken as a whole:

1. There is no significant difference among average abnormal returns to low-leverage lessees, low-leverage non-lessees, and high-leverage non-lessees. However, abnormal returns to high-leverage lessees are found to be negative and significant on average. This is, or appears to be, the interaction effect predicted by the contracting hypothesis.
2. Variation in abnormal performance with respect to "proportion of debt subject to accounting restrictions" is negligible in the case of low-leverage lessees, low-leverage non-lessees, and high-leverage non-lessees. In the case of high-leverage lessees, however, the regression results indicate a significant negative relationship. Moreover, the results imply that, within the latter category, abnormal performance is negligible when proportion of debt subject to accounting restrictions is very small.
3. Variation in abnormal performance with respect to the "public" proportion of debt subject to accounting restrictions is negligible in all of the above mentioned categories of firms.

4. Although a strong and statistically significant "size effect" is evident in the data, the results just described appear to be independent of firm size.
5. Similar results are obtained (a) when the abnormal performance measure is defined to include only those event period intervals in which lessee abnormal returns differed significantly from those of non-lessees; (b) when the present value of noncapitalized financing leases is excluded from the long-term debt measure; (c) when short maturity and floating rate loans are excluded from the measure of debt subject to accounting restrictions; and (d) when certain industry characteristics of lessees are taken into account.

With the exception of the hypothesis concerning the influence of public debt, the pattern of price behavior just outlined is strongly consistent with that predicted by the contracting hypothesis. However, these results appear to be lacking in "robustness" with respect to choice of sample. Non-lessee firms make up roughly half of the sample on which the Chapter 5 tests were based. When the analysis is replicated using the lessee subsample only, the most extreme negative abnormal performance is noted among high-leverage firms with a "moderate" level of noncapitalized financing leases, rather than among high-leverage firms for which lease financing is pervasive. Thus, in the author's opinion, the total sample results may be interpreted as being, but must be construed as no more than, weakly consistent with the predictions of the contracting hypothesis.

To summarize briefly, then, the results of this study are consistent with the hypothesis that the 1973-1976 lessee accounting standard-setting process induced a (relative) downward revision in lessee share price, and weakly consistent with a "contracting"-type explanation for this behavior. Of some interest is whether such a finding bears any policy implications. It is well established that an ex post examination of security price behavior does not provide a basis for conclusions regarding the social desirability of a particular regulation. Thus, the question being raised here is something more on the order of: if a policy maker were concerned about the possibility that a given regulation might induce non-trivial contract-related share price effects, should he be interested in the findings of this study? In the author's estimation, the answer to this question is a qualified "yes." Although the results presented here are relatively weak, they must be considered in relation to those of other studies. It was pointed out earlier in this chapter that two recent studies have produced evidence which is at least indicative of contract-related price effects in connection with other accounting policy processes. While the totality of such evidence is not utterly convincing, it does suggest rather strongly that these effects are both real and material.

Finally, some problems encountered, some phenomena observed, and some qualifications inherent in this dissertation indicate an important area for future methodological research, and two items that should be considered in future policy-oriented research. As to the latter, the results of the present research first suggest that the

"nonstationarity problem" is worthy of serious attention in any study which involves a comparison of contemporaneous returns to an "experimental group" and a "control group." The results also suggest that very high levels of aggregation (of the abnormal performance measure) -- both across firms and across time -- may be necessary if contract-related price effects are to be detected. With respect to the former, it should be emphasized that all of the results reported here are potentially refutable because of the "missing factor problem" discussed in Chapter 2. This problem is present in any study which examines contemporaneous security returns. Since most (if not all) research on the relationship between accounting policy decisions and security price movements is of this type, the missing factor problem effectively precludes inferences as to causation. Thus, there is a most pressing need to develop procedures which can be used to control for the effects of multiple common factors on returns.

APPENDIX

DERIVATION OF BIAS EQUATIONS

Write equation (9) as

$$r = X^0 a + w, \quad (A1)$$

where r is an $(n \times 1)$ vector of returns and w is an $(n \times 1)$ vector of disturbance terms. Consider all variables to be expressed as deviations from their respective means, so that X^0 is an $n \times 2$ matrix of "true" regressors of the form

$$X^0 = [B_j \ z_j]$$

with $B_j = (\beta_j \ -\bar{\beta})$ and $z_j = -k$ for $j \in NL$ and $(1-k)$ for $j \in L$. The ratio k of lessee observations to total observations is assumed to be fixed, and is therefore the mean of z_j . Finally, a is the 2×1 vector of regression coefficients to be estimated.

Now let $X = X^0 + V$, where X is the $n \times 2$ matrix of measured regressors, and V is an $n \times 2$ "bias matrix". In the present problem, $v_{jk} = 0$ except in those positions corresponding to the B_j of lessee firms in X^0 . In the latter instance, $v_{jk} = v$. Equation (A1) can be rewritten as

$$r = Xa + (w - Va), \quad (A2)$$

and the OLS estimator is

$$\hat{a} = (X'X)^{-1}X'r = a + (X'X)^{-1}X'(w - Va). \quad (A3)$$

(A3) can be further decomposed as

$$\hat{a} - a = (X'X)^{-1}X'w - (X'X)^{-1}X'Va. \quad (A4)$$

At this point some assumptions about w are needed. Suppose that the w_j have zero mean and that, in the limit, they are uncorrelated either with the regressor variables X^0 or the bias variables V . In this case, $\text{plim}(X'X)^{-1}X'w = 0$, and

$$\begin{aligned} \text{plim}(\hat{a}) - a &= - \text{plim} [(X'X)^{-1}X'Va] \\ &= - \text{plim} \left[\frac{1}{n}X'X \right]^{-1} \text{plim} \left[\frac{1}{n}X'V \right] a \\ &= - \text{plim} \left[\frac{X^0X^0}{n} + \frac{X^0V}{n} + \frac{V'X^0}{n} + \frac{V'V}{n} \right]^{-1} \\ &\quad \times \text{plim} \left[\frac{X^0V}{n} + \frac{V'V}{n} \right] a. \end{aligned} \quad (A5)$$

$$\text{Now, } \text{plim} \left(\frac{X^0X^0}{n} \right) = \begin{bmatrix} \sigma_{22} & \sigma_{23} \\ \sigma_{23} & \sigma_{33} \end{bmatrix},$$

$$\text{plim} (V'V) = \begin{bmatrix} kv^2 & 0 \\ 0 & 0 \end{bmatrix},$$

$$\text{plim} (X^0V) = \begin{bmatrix} 0 & 0 \\ k(1-k)v & 0 \end{bmatrix},$$

$$\text{and } \text{plim} (V'X^0) = [\text{plim}(X^0V)]',$$

so that (A5) can be expressed as

$$\text{plim}(\hat{a}) - a = - \begin{bmatrix} \sigma_{22} + kv^2 & \sigma_{23} + k(1-k)v \\ \sigma_{23} + k(1-k)v & \sigma_{33} \end{bmatrix}^{-1} \begin{bmatrix} kv^2 & 0 \\ kv(1-k)v & 0 \end{bmatrix} a \quad (A6)$$

Equations (18) follow directly from (A6).

REFERENCES

- Abdel-khalik, A. Rashad, Bipin Ajinkya, and James McKeown, "The Impact of Statement 13 on Stock Returns and Risk Measures," in The Economic Effects on Lessees of FASB Statement No. 13, Accounting for Leases, A. R. Abdel-khalik, Principal Researcher (FASB, Stamford, Conn., 1981).
- Accounting Principles Board, APB Opinion No. 5, Reporting of Leases in Financial Statements of Lessee (American Institute of Certified Public Accountants, New York, 1964).
- , APB Opinion No. 16, Business Combinations (American Institute of Certified Public Accountants, New York, 1970).
- , APB Opinion No. 31, Disclosure of Lease Commitments by Lessees (American Institute of Certified Public Accountants, New York, 1973).
- American Bar Foundation, Commentaries on Indentures (American Bar Foundation, Chicago, 1971).
- American Institute of Certified Public Accountants, Report of the Study Group on Objectives of Financial Statements (AICPA, New York, 1973).
- Andrews, Frederick, "Foes of Lease Accounting Fail to Convince Standards Board of Dangers," Wall Street Journal (November 25, 1974), p. 18.
- Banz, Rolf W., "The Relationship Between Return and Market Value of Common Stocks," Journal of Financial Economics (March, 1981), pp. 3-18.
- Black, Fischer, and John C. Cox, "Valuing Corporate Securities: Some Effects of Bond Indenture Provisions," Journal of Finance (May, 1976), pp. 351-367.
- Black, Fischer, and Myron Scholes, "The Pricing of Options and Corporate Liabilities," Journal of Political Economy (June, 1973), pp. 637-654.
- Bowman, Robert G., An Empirical Investigation of the Debt Equivalence of Leases, Ph.D. Dissertation (Stanford University, 1978).
- Brown, Philip, Allan W. Kleidon, and T. A. Marsh, "New Evidence on the Nature of Size-Related Anomalies in Stock Prices," Journal of Financial Economics (June, 1973), pp. 33-56.
- Cary, William L., "Sale and Leaseback of Corporate Property," Harvard Business Review (March, 1949), pp. 151-164.

- Collins, Daniel W., and Warren T. Dent, "The Proposed Elimination of Full Cost Accounting in the Extractive Petroleum Industry," Journal of Accounting and Economics (March, 1979), pp. 3-44.
- Collins, Daniel W., Michael S. Rozeff, and Dan S. Dhaliwal, "A Cross-Sectional Analysis of the Economic Determinants of Market Reaction to Proposed Mandatory Accounting Changes in the Oil and Gas Industry," Journal of Accounting and Economics (March, 1981), pp. 37-72.
- Committee on Accounting Procedure, Accounting Research Bulletin No. 38, Disclosure of Long Term Leases in Financial Statements of Lessees (American Institute of Accountants, New York, 1949).
- Dieter, Richard, "Is Lessee Accounting Working?," CPA Journal (August, 1979), pp. 13-19.
- Dieter, Richard, and Arthur R. Wyatt, "Get It Off the Balance Sheet!," Financial Executive (January, 1980), pp. 42-48.
- Dyckman, Thomas R., David H. Downes, and Robert P. Magee, Efficient Capital Markets and Accounting: A Critical Analysis (Prentice-Hall, Inc., Englewood Cliffs, N.J., 1975).
- Dyckman, Thomas R., and Abbie Jean Smith, "Financial Accounting and Reporting by Oil and Gas Producing Companies: A Study of Information Effects," Journal of Accounting and Economics (March, 1979), pp. 45-76.
- Fama, Eugene F., "Efficient Capital Markets: A Review of Theory and Empirical Work," Journal of Finance (May, 1970), pp. 383-417.
- , Foundations of Finance (Basic Books, Inc., New York, 1976).
- , and James D. MacBeth, "Risk, Return and Equilibrium: Empirical Tests," Journal of Political Economy (May, 1973), pp. 607-636.
- Financial Accounting Standards Board, Discussion Memorandum: An Analysis of Issues Related to Accounting for Leases (FASB, Stamford, Conn., 1974).
- , Exposure Draft of a Proposed Statement of Financial Accounting Standards on Accounting for Leases (FASB, Stamford, Conn., 1975).
- , Exposure Draft of a Proposed Statement of Financial Accounting Standards on Accounting for Leases (FASB, Stamford, Conn., 1976).
- , Statement of Financial Accounting Standards No. 13, Accounting for Leases (FASB, Stamford, Conn., 1976)

- _____, Statement of Financial Accounting Standards No. 19 -- Financial Accounting and Reporting by Oil and Gas Producing Companies (FASB, Stamford, Conn., 1977).
- Fogelson, James H., "The Impact of Changes in Accounting Principles on Restrictive Covenants in Credit Agreements and Indentures," Business Lawyer (January, 1978), pp. 769-787.
- Fryling, Henry H., "Private Placement Financing - Lenders' Procedural Guide," Proceedings of Association of Life Insurance Counsel (1965), pp. 797-846.
- Galai, Dan and Ronald W. Masulis, "The Option Pricing Model and the Risk Factor of Stock," Journal of Financial Economics (March, 1976), pp. 53-81.
- Gant, Donald R., "Illusion in Lease Financing," Harvard Business Review (March-April, 1959), pp. 121-142.
- Goldman, M. Barry, and Howard B. Sosin, "Information Dissemination, Market Efficiency and the Frequency of Transactions," Journal of Financial Economics (March, 1979), pp. 29-61.
- Hayes, Douglas A., Bank Lending Policies, Domestic and International, 2nd. edition (University of Michigan Graduate School of Business Administration, Ann Arbor, 1977).
- Jain, Prem C., "Cross-Sectional Association Between Abnormal Returns and Firm Specific Variables," Journal of Accounting and Economics (December, 1982), pp. 205-228.
- Jensen, Michael C., and William H. Meckling, "Theory of the Firm: Managerial Behavior, Agency Costs and Ownership Structure," Journal of Financial Economics (October, 1976), pp. 305-360.
- Johnston, J., Econometric Methods (McGraw-Hill Book Company, New York, 1972).
- Keim, Donald B., "Size-Related Anomalies and Stock Return Seasonality," Journal of Financial Economics (June, 1983), pp. 13-32.
- Kmenta, Jan, Elements of Econometrics (Macmillan Publishing Co., Inc., New York, 1971).
- Leftwich, Richard, Private Determination of Accounting Methods in Corporate Bond Indentures, Ph.D. Dissertation (University of Rochester, 1980).
- _____, "Evidence on the Impact of Mandatory Changes in Accounting Principles on Corporate Loan Agreements," Journal of Accounting and Economics (March, 1981), pp. 3-36.

Lev, Baruch, "The Impact of Accounting Regulation on the Stock Market: The Case of Oil and Gas Companies," The Accounting Review (July, 1979), pp. 485-503.

Levi, Maurice D., "Errors in Variables Bias in the Presence of Correctly Measured Variables," Econometrica (September, 1973), pp. 985-986.

May, Robert G., Jeffrey L. Harkins, and Stephen J. Rice, "Lease Disclosure: The Effect of an Accounting Policy Change on Security Prices," working paper (University of Washington, 1978).

May, Robert G. and Eric Noreen, "The Limits of Market Efficiency and Their Implications for Accounting," unpublished working paper (1981).

Merton, Robert C., "On the Pricing of Corporate Debt: The Risk Structure of Interest Rates," Journal of Finance (May, 1974), pp. 449-470.

Miller, Merton, and Myron Scholes, "Rates of Return in Relation to Risk: A Re-examination of Some Recent Findings," in M. Jensen, Ed., Studies in the Theory of Capital Markets (Praeger, Inc., New York, 1972).

Myers, John H., Accounting Research Study No. 4, Reporting of Leases in Financial Statements (American Institute of Certified Public Accountants, New York, 1962).

National Conference of Bankers and Certified Public Accountants, Financial Statement Provisions in Term-Loan Agreements (American Bankers Association, New York, 1968).

Ohlson, James A., "On Financial Disclosure and the Behavior of Security Prices," Journal of Accounting and Economics (December, 1979), pp. 211-232.

Pfeiffer, Glenn M., Accounting for Long Term Financial Commitments: An Investigation Into the Economic Effects of Lease Accounting Changes, Ph.D. Dissertation (Cornell University, 1980).

Reinganum, Marc R., "Misspecification of Capital Asset Pricing: Empirical Anomalies Based on Earnings' Yields and Market Values," Journal of Financial Economics (March, 1981), pp. 19-46.

———, "The Arbitrage Pricing Theory: Some Empirical Results," Journal of Finance (May, 1981), pp. 313-321.

Ro, Byung T., "The Disclosure of Capitalized Lease Information and Stock Prices," Journal of Accounting Research (Autumn, 1978), pp. 315-340.

- Roll, Richard, and Stephen A. Ross, "An Empirical Investigation of the Arbitrage Pricing Theory," Journal of Finance (December, 1980), pp. 1073-1104.
- Rosenberg, Barr, and Vinay Marathe, "The Prediction of Investment Risk: Systematic and Residual Risk," Proceedings of the Seminar on the Analysis of Security Prices (University of Chicago Graduate School of Business, November 1975), pp. 85-226.
- Ross, Stephen A., "Return, Risk, and Arbitrage," in Irwin Friend and James L. Bicksler, eds., Risk and Return in Finance I (Ballinger, Inc., Cambridge, Mass., 1977), pp. 189-218.
- Scholes, Myron and J. Williams, "Estimating Betas from Nonsynchronous Data," Journal of Financial Economics (December, 1977), pp. 309-327.
- "SEC Plans to Require More Information From Firms About Their Leasing Activities," Wall Street Journal (May 18, 1973), p. 9.
- "SEC Adopts Hard Lease-Disclosure Rule But Suspends Merger Accounting Order," Wall Street Journal (October 8, 1973), p. 8.
- Sharpe, William, "Capital Asset Prices: A Theory of Market Equilibrium Under Conditions of Risk," Journal of Finance (September, 1964), pp. 425-442.
- Simmons, Richard S., "Drafting of Commercial Bank Loan Agreements," Business Lawyer (November, 1972), pp. 179-201.
- Smith, Clifford W., and Jerold B. Warner, "On Financial Contracting: An Analysis of Bond Covenants," Journal of Financial Economics (June, 1979), pp. 117-161.
- Sunder, Shyam, "Stationarity of Market Risk: Random Coefficients Tests for Individual Stocks," Journal of Finance (September, 1980), pp. 883-896.
- U.S. Securities and Exchange Commission, Securities Act Release No. 5401, in SEC Docket (June 19, 1973).
- , Accounting Series Release No. 147 in SEC Docket (October 23, 1973), pp. 545-548.
- , Accounting Series Release No. 225 in SEC Docket (September 12, 1977), pp. 1612-1617.
- , Accounting Series Release No. 235 in SEC Docket (January 10, 1978), pp. 1209-1210.

Vancil, Richard F. and Robert N. Anthony, "The Financial Community Looks at Leasing," Harvard Business Review (November-December, 1959), pp. 113-130.

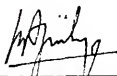
Watts, Ross L., and Jerold L. Zimmerman, "Towards a Positive Theory of the Determination of Accounting Standards," The Accounting Review (January, 1978), pp. 112-134.

Zinbarg, Edward D., "The Private Placement Loan Agreement," Financial Analysts Journal (July-August, 1975), pp. 33-52.

BIOGRAPHICAL SKETCH

Robert B. Thompson II was born on August 5, 1949, in Tampa, Florida. After an uneventful childhood and two years' service in the U.S. Navy, he entered the College of Business Administration at the University of Florida, earning the degree of Bachelor of Science in Business Administration (with honors) in 1976. He entered the Ph.D. program in the University of Florida Graduate School of Business Administration in 1977, and was admitted to candidacy in 1979 after passing qualifying examinations in accounting (major field) and finance (minor field). Mr. Thompson is a Certified Public Accountant and is currently employed as Instructor in Accounting at the University of Texas at Austin.

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.



Bipin B. Ajinkya, Chairman
Associate Professor of Accounting

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.



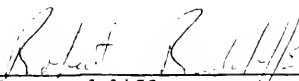
A. Rashad Abdel-khalik, Co-Chairman
Professor of Accounting

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.



E. Daniel Smith
Professor of Accounting

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.



Robert Radcliffe
Associate Professor of Finance,
Insurance, and Real Estate

This dissertation was submitted to the Graduate Faculty of the Department of Accounting in the College of Business Administration and to the Graduate Council, and was accepted as partial fulfillment of the requirements for the degree of Doctor of Philosophy.

April 1984

Dean for Graduate Studies and
Research

